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ON
TRANSVERSE FRACTURES
OF THE
PATELLA.

BY
JONATHAN HUTCHINSON, F.R.C.S.,
SURGEON TO THE LONDON HOSPITAL.

*[From Volume LII of the 'Medico-Chirurgical Transactions,' published
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Received May 17th.—Read June 22nd, 1869.

FOR some years fractures of the patella have claimed my especial attention, on account of certain features which seem to me to have been somewhat misunderstood. I have had large opportunities for examining them, and have done my best to follow up my cases until their final results became known. Some errors have, I think, become current owing to the result being recorded at much too early a date, and more especially has this been the case as regards “bony union.”

On the other hand we are liable to mistakes if we venture to found opinions on museum specimens, to which no life history is attached. My aim has been to connect the circumstances observed in the early stages of these injuries with the conditions discovered in the patient's after-life, or revealed by post-mortem dissection, and thus to get a complete view of the whole case.

With a view to conciseness I have stated my conclusions in a series of propositions. Some of these only express the

general creed of surgeons, whilst others more or less differ from it.

PROP. I.—For the purposes of clinical discussion fractures of the patella may be conveniently divided into those in which separation between an upper and lower fragment is possible, and those in which it cannot occur.

PROP. II.—The cases in which no separation of a distal and proximal fragment is possible, are much fewer in number than the others, with which they have scarcely any feature in common.

In this group we count cases of vertical fracture (probably very rare); those cases of starred fracture from blows in which no mobility is possible, and in which probably the articular surface of the bone is not involved; and the not infrequent cases in which fragments of greater or less size are broken off from the borders of the bone.

In none of these can any important questions of treatment arise, it being clear that the limb should be kept at rest, and probable that in many instances bony union will result.

PROP. III.—The cases in which separation between the upper and lower fragments is possible, are common, and occur especially in adults who follow laborious occupations.

PROP. IV.—The line of fracture is usually transverse or nearly so, and often near the middle. If one fragment be larger than the other, it will probably be the upper one. Sometimes the lower one is very small. In a few cases the bone is broken into three or more fragments.

PROP. V.—These transverse fractures are not infrequently caused by muscular contraction alone.

PROP. VI.—In the numerous cases in which direct violence is applied to the bone at the moment of fracture, there is almost always muscular contraction simultaneously present, and it is impossible to say which takes the chief share in the result.

The patella probably never breaks transversely except when the knee is more or less bent, and the bone balanced as a lever on the condyles of the femur. Sudden and unexpected flexion, or the necessity to prevent such, is more likely to break it than extreme flexion. The position of the fracture may serve roughly to indicate the degree of flexion existing at the time the fracture happened. If the fracture is very low down, probably the knee was but little bent, and thus the greater part of the bone still resting on the condyles. I have met with not a few cases in which, as far as the patient's knowledge went, no violence whatever was received on the bone at the time of fracture. In a majority, however, there is a history of the knee having been struck against something during the fall.

PROP. VII.—If the bone is broken by direct violence at a time when the muscle is not in action, there will be no material separation of the fragments either at the time or subsequently; and the probability is very great that the line of fracture will not be transverse.

Unless the bone be held firmly as a lever by the muscles at the time it receives the blow, there is no reason why the fracture should cross the bone transversely, and the probability is that it would be starred. The fact that most fractures are transverse is therefore an argument in favour of the current belief that muscular action is a main agent in causing them. The fact that direct violence under such circumstances cannot possibly rupture the strong fibrous structures by which the bone is everywhere surrounded, explains the circumstance that no subsequent separation is likely to occur.

PROP. VIII.—The existence of a wide separation proves that the muscle was in vigorous contraction at the time the bone was broken, and also that the fibrous structures have been extensively lacerated.

The fibrous structures at the sides of the patella are especially strong and are firmly united with it. To admit

of wide separation there must necessarily be a considerable laceration into the front of the joint. Such laceration is possible only at the time of the accident.

PROP. IX.—In all cases of transverse fracture involving the whole thickness of the bone, the knee-joint is necessarily implicated, and it is probable that blood will find its way into the joint, and that synovitis with effusion will follow.

The quantity of blood in the joint is probably often very small, but in severe cases it may be considerable. In one case under my observation it distended the joint, and it was necessary ultimately to evacuate it by incision. The effusion of synovia is an almost constant result, and is, I believe, a very important complication of the case.

PROP. X.—There is no persistent contraction of the quadriceps extensor, or of any part of it, after transverse fracture with separation. When the spasmodic effort which attended the fracture ceases, the muscle usually remains completely and permanently at rest.

My experience has invariably been to find the front of the thigh soft and flabby, the quadriceps being quite at rest. I have scarcely ever known the muscle pass into spasm during examination, but in reference to this remark I must state that it does not apply to the first few hours after the accident, respecting which my knowledge is very limited.

PROP. XI.—The subject of a transverse fracture of the patella is usually quite unable by volition to put the muscle into action, and there is no reason to believe that the muscle is liable to attacks of accidental spasm.

If you ask a man with a recently broken knee-cap to try to lift his limb from the bed, he will reply helplessly, "I cannot;" if you urge him he will get emphatic and

declare, "Sir, I couldn't do it if you gave me the world," but all the time his quadriceps will make not the slightest movement.

PROP. XII.—After transverse fracture with separation, the fragments become of course widely parted if the knee be bent. If, however, the knee be kept straight, their relative position will not be in the least altered by lifting the limb from the couch and elevating it at right angles with the trunk.

The degree of separation produced by bending the knee after transverse fracture varies considerably in different cases. In some the upper fragment does not follow downwards at all, owing probably to extensive laceration in the fibrous structures. In others, although the fragments are quite movable laterally, there is scarcely any gaping in flexion. The experiment is not one which it is wise to try often, nor except with the utmost gentleness. It is not justifiable except in the earliest stage.

PROP. XIII.—The bringing down of the upper fragment is not as a rule in the least facilitated by the common practice of elevating the limb.

In an uninjured limb at rest in the straight position the patella can be moved upwards or downwards for about three fourths of an inch, the ligamentum patellæ restricting its movement upwards, and the fascia preventing its further passage downwards, whilst the rectus muscle remains perfectly flaccid and offers no sort of impediment. Exactly the same condition of things is met with after transverse fracture. I have tried the experiment of lifting the limb dozens of times, and with great care, but never found the upper fragment to alter in position in the least. Care must of course be taken to make the movements gently, and not to let the knee bend. For years I have never treated fractures of the patella except in the longitudinal position.

PROP. XIV.—The causes of difficulty in bringing the upper fragment down are several.

1st.—Effusion into the joint lifting the fragments apart.

2nd. Swelling of the torn soft tissues.

3rd. Contraction of the fibrous structures (chiefly met with when the fragments have been pushed apart by swelling which has lasted for some considerable time).

4th. The difficulty there is in getting well hold of the upper fragment, especially when much swelling is present.

Of these probably by far the most constant and the most efficient cause is effusion into the joint. If you examine a case in which there is little or no effusion, there will be little or no difficulty in bringing the fragments close together. In such cases also it is probable that the laceration is but slight, for the laceration, and the effusion which results, will usually be in relation.

The effect of distension of the joint from within in lifting apart the fragments can easily be imagined, and no doubt effusion into the cellular tissue, between the layers of fascia, &c., will do something towards making the parts rigid, and thus in preventing approximation.

In proportion to the external swelling is the difficulty of getting external pressure, whether manual or by pads, to act efficiently on the upper fragment. The influence of pressure on the sides of the distended joint in making the fluid bulge upwards into the laceration must never be forgotten.

PROP. XV.—In the majority of cases it is easy to force the fragments close and thus obtain crepitus.

PROP. XVI.—The difficulty of securing them in apposition will be exactly in proportion to the amount and duration of the swelling.

I have found much less difficulty in pressing the fragments together, so as to produce crepitus, than books would

lead us to expect. If difficulty has occurred, it has been from the swelling.

PROP. XVII.—The muscle scarcely ever offers any impediment to the approximation of the fragments.

I do not recollect a single case in which, after the first day or two, even forcible pressure on the upper fragment excited any degree of muscular contraction.

PROP. XVIII.—No proof has yet been afforded that transverse fractures of the patella with considerable separation at the time ever unite by bone.

The only proof which we ought to accept on this point is the section of a specimen concerning which the history is definite. There may be such, but I do not know where to refer to them.

PROP. XIX.—It is quite impossible on the living subject to ascertain whether or not bony union has occurred.

In many of my cases at the time of the patient's discharge from the hospital, the fragments were so firmly bound together that I could not produce the slightest lateral movement. In one or two of such cases I have found, by the subsequent stretching of the uniting medium, that it certainly was not bone. A specimen in the London Hospital museum shows such close union, that without section it would have been impossible to say that it was not bone.

PROP. XX.—Transverse fractures with separation may have several different events.

1st. and best.—Close and permanent union of the fragments by fibrous tissue, so that no movement whatever can be detected, and so that section of the bone is needed to show that it is not true bony union.

2nd.—Union by a short and strong ligament.

3rd.—Union by a long ligamentous band which may stretch indefinitely.

4th.—Entire absence of any uniting medium between the bones; the fibrous capsule of the joint, bursa, &c., being the only means of connection.

After six weeks' rest and ordinarily careful treatment by back-splint and strapping, it is, I think, easy to obtain close and firm union, in a fourth or even a third of the cases. In another third the union will be close and strong, but still, on forcible pressure, permitting of slight lateral movement, whilst in the remainder union will be weak and with some persisting separation. In the worst cases the separation ought not at this period to be more than a quarter of an inch.

All statements of results made at the date of the patient's leaving the hospital are utterly untrustworthy, for in some of the best cases the uniting medium will afterwards stretch. The real result is not known until the patient has been allowed to walk about and bend his knee. A year's interval, at least, ought to be allowed, and even after this it is possible that the fragments may become much more distant.

The assertion that non-union, or membranous union, is a frequent result is, I think, a fallacy.

In all the cases that I have examined at the expiration of treatment, there has been reason to believe that the fragments were connected by a fairly thick uniting medium. Thus they could not be separated beyond a certain length, nor could much lateral movement be produced.

The specimens so common in our museums of wide separation, and in which the thickened capsule, bursa, &c., are the only connecting structures, are, I suspect, from cases in which premature flexion had been permitted, and the new material stretched until practically lost.

PROP. XXI.—The chance of the best result is greatest in cases in which but little swelling is present, and the fragments are easily approximated; but it by no means always follows that, in cases in which the fragments easily lie quite close, firm union will result.

PROP. XXII.—The causes of weak union, or of non-union, are probably two.

1st. The fact that the fragments are usually at some little distance from each other during the first week or two of treatment.

2nd. That the space between the fragments is occupied by a fluid secretion (synovia).

Synovitis is an almost invariable consequence of transverse fractures of any degree of severity, and its effusion usually lasts ten days or a fortnight. During this period approximation is difficult and in many cases cannot be attempted.

Without attributing any special effect to synovia, as synovia, in preventing union, it can easily be understood that the presence of any fluid between the ends of a fractured bone must tend in a most important manner to prevent the consolidation of new material. It will supply a menstruum in which the lymph-cells may be washed away and the plasma dissolved.

PROP. XXIII.—Amongst the causes which help to make actual bony union so exceedingly infrequent, it is, perhaps, fair to suggest that the patella differs from most other bones in having periosteal fibres on one surface only, and that from the direction in which the fracture-force is always applied, all those fibres must invariably be ruptured.

PROP. XXIV.—The objects to be attended to during the treatment are—

1st. To keep the knee straight.

2nd. To get rid, as quickly as possible, of all effusion from the joint.

3rd. To avoid all pressure on the sides of the joint, since such pressure must inevitably force the synovial fluid to bulge forwards into the laceration, and thus separate the fragments.

4th. To secure the lower fragment as high, and the upper one as low, as possible.

If the effusion be but little, an attempt may at once be made to secure co-aptation; but if otherwise it is better, I think, to wait a while and to use ice to hasten absorption.

PROP. XXV.—Some unexpected results—hitherto, I believe, not described—frequently occur and in some of the best cases prevent the hopes of the surgeon from being fulfilled. Amongst these it may be mentioned that the quadriceps muscle undergoes a very remarkable wasting during the patient's confinement to bed. This wasting is altogether out of proportion to that of the other muscles of his limb and is almost invariably attended by corresponding loss of power.

The wasting of the extensor muscle is, I believe, an almost constant result. If you measure the two thighs, that of the injured side will be much less in girth than the other, and examination will show that the loss of substance is almost wholly in front, the flexors being still plump and large. The patient will be able very easily to put his flexors in action, whilst he will be almost unable to attempt extension, or will do so in a very feeble manner. In many cases he will be absolutely unable to do so, and attempts to lift the limb will result in his bending the knee a little, and then accomplishing a slight elevation chiefly by means of the sartorius.

It is clear that mere disuse will not account for these results; for the flexors must have been, equally with the extensor, at rest during treatment. The explanation must probably be sought in the same physiological law which disables the patient from calling the muscle into action in the early part of the case.

PROP. XXVI.—In probably more than half of the cases of

transverse fracture of the patella, the quadriceps remains ever afterwards thin and weak, whilst in a not inconsiderable number, it is completely atrophied.

Even in the best cases,—in which the patient regains good power over the extensor and can lift his leg, or kick vigorously,—it is still usual to find the muscle much less bulky than its fellow. In a large proportion the loss of bulk on the front of the thigh is perceptible both to the eye and the hand. I have met with at least six cases of absolute atrophy, so that the bone was quite bare. Most of these were old cases which had been variously treated, but one was in a patient of my own. I do not believe that details of treatment have anything to do with causing or preventing this curious result.

PROP. XXVII.—In almost all cases of fracture with separation, the structures connected with the upper fragment acquire a degree of rigidity during the treatment, which afterwards prevents the upper fragment from easily passing downwards in flexion of the knee.

The condition referred to is, I suppose, one of atrophic contraction. Hunter thought that when separation of the fragments resulted, the extensor muscle must remain too long for its work, and that the patient had to be educated to unusual exertion of it in order to make it act upon the patella and tibia, but, probably, the reverse is the case, and atrophic shortening is the real result.

PROP. XXVIII.—Thus the real danger as to stretching a ligamentous union, is in letting the patient *bend the knee*, there being little or no danger in allowing him to use his quadriceps.

The function of the extensor muscle has been greatly diminished, both as regards ability to contract and also to relax. It can do neither well. It can neither allow the patella to move downwards in flexion, nor draw it much upwards in extension. After a time it will improve in both

respects; but, meanwhile, if flexion have been permitted, the uniting ligament will certainly have been stretched.

PROP. XXIX.—Unless the surgeon has in any case deliberately abandoned all hope of benefit from keeping the ligamentous union short, he ought, on no account, to permit even passive flexion of the knee.

Several authorities, more especially Mr. South (see his edition of Chelius), recommend systematic flexion in the sitting posture at an early period, in order to prevent stiffening of the joint. If my conclusions are correct such exercises are certain to stretch the medium of union, and that often very rapidly. I learned the danger from observing this result.

PROP. XXX.—In any case of weak union the surgeon has his choice,—

1st. To let the patient have motion at the knee with the inevitable result of stretched ligament, or

2nd. To keep him permanently with a straight knee and wearing an apparatus.

It may be doubted whether there is any third possibility.

I apply these propositions to all cases in which the fragments are united by fibrous tissue, but not so firmly but that lateral movement can be produced between them. In all such, if flexion is permitted within six months, the uniting structures will stretch, and I do not know that the liability does not extend much longer.

PROP. XXXI.—In cases of close and firm fibrous union (so-called “bony unions”) it will be wise to try flexion cautiously. If the patella moves downwards easily then probably it will be perfectly safe.

If the limb can be bent without giving pain, and the whole patella carried downwards as one bone, then the

result is equivalent to bony union, and probably no ill results will follow from use.

PROP. XXXII.—After complete atrophy of the quadriceps a person can walk very well, and may follow almost any occupation.

The main risk is that owing to the weakness of the limb another fall may occur, and the other patella be broken.

I have met with patients in this condition who could walk twenty or thirty miles a day, and one who was a van-driver and accustomed, with care, to clamber up and down to his box without material inconvenience.

PROP. XXXIII.—The main question for the surgeon in deciding as to whether to advise a patient with weak union to bend the knee or to keep it straight and supported permanently by an apparatus, is as to which plan is the most likely to prevent falls.

The convenience in walking will be found almost equal.

My impression is, that the use of external support adds materially to the patient's safety, and often to his comfort also. I have met with patients, however, who preferred a thin bandage or an elastic knee-cap to a stronger support.

PROP. XXXIV.—The frequency with which the quadriceps atrophies after transverse fractures, suggests the desirability of using galvanism from the first to maintain its nutrition. If used, the limb should, during the process, be raised to the utmost, and the patient made to sit up in bed.

Recapitulation.

Thus then, briefly to recapitulate, I have tried to show—

1st. That a very remarkable state of physiological

inactivity in the damaged muscle results from fracture of the patella.

2nd. That the fact of this inactivity destroys the common explanation of the displacement of the upper fragment.

3rd. That this displacement is usually due, not to muscular traction from without, but to fluid pressure from within.

4th. That the real cause of weak union is the bulging of fluid between the fragments.

5th. That atrophic shortening of the quadriceps often follows these injuries, and, whilst it greatly disqualifies the muscle for extension, renders flexion very dangerous to the uniting ligament.

6th. That an exceedingly interesting and curious occasional result is, permanent and complete atrophy of the muscle concerned.

Should the conclusions which I have stated be confirmed by future experience, it is probable that they will also be found applicable to some other forms of fracture and of injury to tendons. Indeed, it is in the possibility that they may illustrate a physiological law of wide bearing that their chief interest consists.

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A SECOND REPORT
ON THE
COMMUNICATION OF SYPHILIS
IN THE
PRACTICE OF VACCINATION.

WITH TWO ADDITIONAL CASES.

BY

JONATHAN HUTCHINSON, F.R.C.S.,

SENIOR SURGEON TO, AND LECTURER ON SURGERY AT, THE LONDON HOSPITAL;
SURGEON TO THE MOORFIELDS OPHTHALMIC HOSPITAL AND TO
THE BLACKFRIARS HOSPITAL FOR SKIN DISEASES.

*[From Volume LVI of the 'Medico-Chirurgical Transactions,' published
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IN the spring of 1871 it became my duty to ask the attention of the profession, in the most prominent manner that I could, through the medium of this Society, to two series of cases in which syphilis had been communicated to numerous individuals in the practice of vaccination. There was nothing new to medical science in the nature of the facts, for closely similar ones had occurred previously in various parts of the Continent, and had been faithfully recorded. The narratives of them had also excited much attention amongst ourselves, and had been criticised and investigated by many writers, amongst the ablest of whom I must mention the name of Dr. Ballard, who in his work on Vaccination had treated the subject exhaustively, had avowed his conviction of the reality of the occurrence, and had anticipated almost all the observations which have

occurred in connection with subsequent cases. Nor had we in English practice been wholly free from such accidents, for, not to mention some more or less suspicious occurrences which were not placed beyond doubt, a case which was perfectly clear in all its details had been brought by Mr. Thomas Smith before the Clinical Society six months prior to my own paper. Notwithstanding these circumstances, however, I believe that the facts which I brought before the Society produced in the British profession a sense of most disagreeable surprise, and awoke many for the first time to the perception of a danger which they had never before realised, and of which most had scarcely credited the existence. In the minds of some, I believe that I incurred blame, as being unwise and rash in making such facts public. I could not myself think so at the time, nor do I now, for it seemed most necessary that the knowledge of such a danger should be diffused as promptly and widely as possible. This opinion has been confirmed by the circumstance that during the last eighteen months two other examples of the communication of syphilis in vaccination have come under my notice. One of these presented itself amongst my patients at Moorfields, where the man applied on account of iritis and without the slightest idea as to what he was suffering from, and the other was brought to my house as a private patient, the lady being the subject of a urethral growth. I wish particularly to state that neither of the cases was sent to me because I had taken an interest in vaccination-syphilis, but that both showed themselves in the ordinary course of practice. It may be recollected that neither of my first series came under observation in this way, but that in both the nature of the disease had been recognised by others before I was consulted. I purpose in the present paper to relate briefly the particulars of the two cases to which I refer, and neither of which has as yet in any way been made public, and after doing this I shall ask to be allowed to make a few general remarks on the whole of the facts which I have had under observation. I may just add that during the last two years several cases have been sent

to me as supposed examples of vaccination-syphilis, but in none could the diagnosis be supported.

THIRD SERIES (ONE CASE).

The following case came under my care at Moorfields, where its subject applied on account of acute iritis, of the cause of which he had no suspicion.

The patient was a tobacconist, æt. 46, a married and respectable man. The iritis for which he applied at Moorfields was double. On investigation it was found that his body and limbs were covered with a papular and scaly rash of a very definitely syphilitic nature; he had also symmetrical sores in the tonsils. The examination further revealed the fact that he had large unhealed sores on his arm which had resulted from vaccination. He told us that he had been vaccinated three months before, being at the time in perfect health. Several of his children (young adults) were vaccinated at the same time and from the same child.

His account was that the vaccination places took and went on favorably, but that just as they were about healed, during the fourth week, they again inflamed, became ulcerated, and gave him much trouble. He attributed this relapse of inflammation to the irritation of tobacco dust, in which he worked. It was not until more than six weeks after the vaccination that the eruption showed itself, and not till the end of another month still that the iritis occurred. He had not as yet had any specific treatment, since the nature of his disease had not been recognised. On being questioned as to the possibility of his having contracted syphilis he firmly denied it. He readily submitted to an examination of his genitals, and we could not find the slightest trace of sore on them. His vaccination sores, at the time he came under my notice, were open ulcers as large as shillings, covered with scab and with dusky indurated borders. There was an indolent swelling in his armpit.

This man remained under my treatment at Moorfields

for two or three months, and was seen repeatedly by many members of the profession who were attending my clinique. There could not be the slightest doubt that he was suffering from secondary syphilis, and that too in a very severe form. We treated him with mercury, and both his iritis and rash disappeared.

Everything in the man's history that we could ascertain seemed to point to vaccination as the source of contamination. His vaccination spots were the seat of chancrous induration after having in the first instance healed. The date of their induration and the date of appearance of the rash fitted exactly with the hypothesis that he had in some way acquired syphilis about the date of his vaccination.

Having obtained from him the name of his vaccinator I called on that gentleman, and from him obtained information which strongly confirmed the man's statements. He said that he had never in his life seen such vaccination sores as this man's arm displayed. "He had been quite frightened at them," and had thought their condition so near phagedæna that on two occasions he had applied a strong solution of nitric acid. He had never, however, suspected the real nature of the disease.

About a dozen other persons had, I was informed, been vaccinated at the same time and from the same child as our patient. The vaccinator told me that he knew them all, and that, with the exception of a little trouble in the healing of the sores in one or two, none of them had shown anything peculiar. He was very anxious that we should not excite alarm in the neighbourhood by instituting any inquiry about these patients, and as he promised to get to know quietly how they were doing and to inform me if he found anything suspicious, I thought it best not to press the matter further. I ascertained from our patient himself that his own children, three in number, who had formed part of the series, had all done perfectly well. The vaccinator told me that the infant from whom he had vaccinated was a model of vigour and health, and he courteously procured for me an opportunity of seeing it.

The Vaccinifer.—I found it a very large and very fat baby, eight months old. It had no rash nor any trace of condyloma, and the only point about it suggestive of syphilis was the state of the bridge of the nose, which was decidedly broad and sunken. Its mother appeared in good health, but she told me that her first two children had died in infancy and that this, her third, was the only one she had living.

I have, in my own mind, no doubt from the appearance of the child's nose that it is the subject of inherited taint, and shall confidently expect that future years will prove the correctness of this suggestion; nor do I see any other way in which the symptoms displayed by the man can be accounted for, excepting on the supposition that he contracted syphilis from vaccination. If we suppose that from some other source he acquired a chancre at or near the date of his vaccination, it is remarkable that no trace of such sore should have been left; and it is yet more remarkable that the vaccination spots should have become indurated and have caused an axillary bubo. Nor does it seem in the least probable that the vaccination sores had become accidentally contaminated. No plausible source of such contamination can be suggested, while, as already stated, the dates fit exactly with the supposed introduction of the virus at the time of vaccination. Thus, we seem to have an instance in which only one patient out of twelve vaccinated from the same subject became contaminated; at any rate there is, as yet, no proof that any of the others have suffered, but under the peculiar circumstances of the case I can by no means accept this absence of evidence as conclusive. It is not improbable that something more may in the future be heard of some of these patients.

I will now relate the particulars of the fourth example of vaccination-syphilis which has come under my notice. They are in most points very similar to those of the preceding case; out of three vaccinated only a single individual became diseased.

FOURTH SERIES (ONE CASE).

Mrs. M—, aged about 46, came under my care last December. Her ailment was a vascular growth in the urethra, but in the course of the examination I found that she was covered with the stains of a syphilitic rash. On my asking about it she told me that she had been very ill after vaccination and had suffered from a severe eruption and from inflammation in one eye.

The following, on inquiry, proved to be the facts of her case:—She was vaccinated in May, 1871, and her two daughters, one aged 30 and another 15, were done at the same time. Four punctures were made in her arm and all of them soon healed, not having taken; but a month later one of them again became sore, and a hard-edged very troublesome ulcer resulted which lasted for three months. Within a few weeks of the formation of this ulcer, *i. e.* within seven or eight weeks of the date of the vaccination, she became covered with rash. It was especially copious on the chest and back of the neck and affected also the palms of her hands. From the date of the vaccination in May until the beginning of September she remained under the care of her own medical attendant by whom the vaccination had been performed. In September, as the rash remained out, she consulted another practitioner. Under her second adviser she remained for nine months taking almost continuously iodide of potassium and bichloride of mercury in small doses. In July, 1872, when the rash had been out eleven months and was still but half cured, iritis occurred in the left eye. It was so severe as to threaten the destruction of the organ, and after some delay she got admitted into the Ophthalmic Hospital. It should be stated that for two months before the outbreak of the iritis she had suspended all treatment and had been staying at the sea-side for the benefit of her health. She was in Moorfields from July 19th to August 1st and was treated by mercury and atropine. The diagnosis given, as I ascer-

tained from her prescription paper, was "Iritis, specific (?), after vaccination (?)" At the time of the iritis her rash relapsed and her palms became covered with scaly patches, her finger-nails also thickened.

At the time that this patient came under my observation she was much out of health and was still covered by the stains of an eruption about the nature of which there could not be the slightest doubt. There were adhesions of the iris in the left eye. I found that she had been much annoyed with the gentleman who had vaccinated her, attributing what had followed to his having done it when she was not in a proper state of health. She had not, however, the slightest idea of the nature of the disease from which she had suffered. Her surgeon had not suspected it either, and consequently, during the first four months, no specific treatment had been adopted. On asking her as to the particulars of the vaccination she told me that it was done from arm to arm in the surgeon's own house. The infant, according to her statement, was puny and its mother looked ill. She was not aware whether the child's arm bled or not. In her two daughters who, with her, were vaccinated from the same child, no definite ill results followed; in both, several spots took, went through their stages and healed soundly at the usual time. In neither of them did any of the spots re-ulcerate, but the younger one twice suffered from transitory rash, probably urticaria. I afterwards called on the surgeon who had vaccinated this patient: he confirmed her statements in all particulars excepting that he did not allow that the vaccinifer looked puny.*

The Vaccinifer.—I succeeded in tracing the vaccinifer. Its mother was the wife of a lighterman and had borne three children previously. Of these the eldest is a boy of ten who now shows no signs of syphilis and who is said to have had no symptoms in infancy. The second was stillborn. The third was a girl of four who had no special symptoms in

* It seems probable that the woman who is the subject of our narrative was the last or last but one vaccinated out of a considerable batch.

infancy excepting that for several months during teething she had very troublesome ulcers at the anus (condylomata). For these she was long under medical care and had numerous local applications. At the same period she was considered to have "a tendency to water on the head." The vaccinifer was four months old at the date of vaccination and appeared, according to his mother's statement, to be perfectly well. Subsequently, however, when dentition commenced he, like his elder sister, had very troublesome sores at the anus and a tendency to water on the head. For the anus he was three months under treatment at a dispensary and had blue stone applied. He is living, full grown, and shows no peculiarities excepting a large forehead.*

There cannot be the slightest doubt in this case that the patient has suffered from syphilis, the only question to be raised is how was it obtained? On this point I hold that a vaccination puncture having reopened a month after healing, having formed a large hard-edged ulcer which lasted three months and which was promptly followed by the rash, is an almost conclusive piece of evidence. The dates of each occurrence are precisely what we should have expected.

The patient is a married woman past middle life, and there is not the least reason for suspecting contagion from any other source. I may add that the vaccination ulcer has left a large dusky scar exactly such as I have seen in other cases of vaccination-syphilis.

Before leaving this case I may ask attention to the fact that the secondary symptoms were unusually severe and protracted, a circumstance not improbably due to the fact

* Since this paper was written I have obtained from the mother the name and address of a surgeon who treated her child. On inquiry he tells me that the child had *syphilitic condylomata*, and was, on and off, under his care for months. He states that it had also snuffles and a slight skin rash, and that it was cachectic and puny. He did not ask the parents any direct questions because he was perfectly certain as to the nature of the ailment.

that no specific measures were employed during the first four months, and that they were subsequently given very insufficiently.

I will now, with the permission of the Society, make a few general remarks on the subject of vaccination-syphilis, chiefly in connection with the four series of facts which I have recorded. It will be convenient to arrange what I have to say under separate headings.

1st. What are we to infer from the circumstance that when syphilis is conveyed in the practice of vaccination it does not affect all of those vaccinated from the tainted source? Clearly, I think, we must believe that the specific poison of syphilis is either not contained in the vaccine lymph at all, or is not equally diffused through it. In my first series of cases two patients out of twelve were successfully vaccinated and wholly escaped syphilis; in the second series out of about twenty-six more than half escaped; and in the third only one out of twelve is known to have suffered, whilst in the fourth only one suffered and six or eight probably escaped.

In the first and second series it was repeatedly observed that of those who contracted syphilis, some of the vaccination punctures developed chancres and others did not. There cannot be the slightest doubt that it is quite easy to vaccinate from a tainted vaccinifer without conveying syphilis, and on the other hand that it is possible to convey syphilis either with or without the production of a normal vaccine vesicle. Now, the supposition that it is necessary to convey some of the cell-elements of the blood in order to convey syphilis, seems to my mind by much the most probable explanation. Probably it is not necessary that these elements should be visibly red. That the vaccine virus itself in a pure state cannot produce syphilis seems highly probable, since in several recorded instances vaccination has been inadvertently performed on a considerable scale from a child that was subsequently found to be syphilitic,

and without ill consequences. It is probable that in a great number of instances, in addition to those placed on record, this has happened, and the evidence supplied by it in reference to the impotency of pure vaccine lymph in the production of syphilis is very strong. On the other hand experiment has fully proved, and more especially the well-recorded experiment of Professor Pelpizzari, that the blood of a patient in the secondary stage of syphilis can, when inoculated, produce a chancre which will be followed by the usual role of syphilitic phenomena. The facts in the case referred to afford as regards dates, &c., a very exact parallel with what was observed in all the cases which I have recorded.

Next we may ask is it absolutely necessary that *blood* should be used in vaccination in order to convey syphilis? It seems highly probable that it is not. At any rate there is not the least evidence in three out of the four series of cases which I have recorded that the lymph used was visibly contaminated with blood. The vaccinator in each instance asserts that it is his habit most scrupulously to avoid making the vesicle bleed. Probably it is quite sufficient to allow the vesicle to draw or weep. With this drainage no doubt corpuscular elements of the blood and tissues become free. According to this supposition as soon as the first contents of the vesicle are exhausted the risk begins. It is well-known that it is the custom of many experienced vaccinators to allow the ruptured vesicle "to weep," and to continue to employ its secretion long after the exhaustion of its original elements.

If the syphilitic virus and the vaccine virus be implanted at one and the same time, what will be the course of events? The cases recorded show conclusively that, if the patient be susceptible to vaccination, the vesicle may pass through all its stages in the most characteristic manner. Then after healing of the vaccination-sore, and at the end of about a month from the inoculation, the syphilitic virus begins to show its effects and the scar becomes irritable, inflames, and indurates. Although this course is the usual one it is not

invariable and deviations from it may be observed in connexion probably with the patient's age, state of health, and condition of tissues.

In these exceptional cases the vaccination-sore never heals, and the pus-scab which forms over it combines with the inflammatory swelling around to conceal the nature of the specific changes which subsequently occur. Should the vaccination not have taken, it is usual for the puncture to heal and for the patient to think no more about it until induration occurs at the end of the month.

What are the usual characters of the vaccination-chancere ?

As already hinted above, the amount of inflammatory effusion on the surface of the sore, and of inflammatory œdema at its base, may in certain cases be considerable. In several of the cases in my second series the specific characters of the chancere were in this way quite concealed. In these instances the patients were children. In the man who is the subject of my third observation the history was that the sore had been very acutely inflamed, so much so that the surgeon several times cauterised it, and probably it was on the verge of phagedæna. These conditions are, however, exceptional and in a usual way the vaccination-chancere shows but little tendency to excess of inflammation. In some cases it does not even ulcerate. It begins as a little red, firm, glossy tubercle which gradually increases in size and becomes harder. At the end of a fortnight, or earlier, it usually ulcerates and presents a sore remarkable for its small amount of secretion and for the hardness of its base and edges. The cases in which no mercury was given show that it may last for some months before it heals. After healing it leaves a dusky brown scar very different indeed from that of vaccination.

What treatment ought the vaccination-chancere to receive ?

I can feel no doubt that should a vaccination-scar take on the induration characteristic of a chancere, and should the other facts of the case corroborate the suspicion, it is the surgeon's duty without delay to begin the administration of mercury. The cases which I have recorded show in the

strongest possible light the great difference in result between those in which mercury was given and those to which nothing was done. In my *first series* of cases the nature of the accident was recognised during the sixth week after vaccination and prior to the occurrence of any well-marked secondary symptoms. In all the patients, excepting one, mercury was at once commenced and in all these the progress of the chancre was at once arrested and rapid cure resulted. For a considerable period no secondary symptoms shewed themselves, and the success of the treatment was such as to induce not a few to doubt the correctness of the diagnosis. Subsequently, however, secondary symptoms showed themselves in several of the patients. They were so well characterised as to put all scepticism about the nature of the disease out of question, but still they were comparatively very slight. They yielded very quickly to the renewed administration of mercury and none of the patients in any material degree lost health either from the disease or the remedy, iritis did not occur in any single one and I believe they are all at the present date quite well. The only case which gave any real trouble was that of a young woman in whom suppuration in the cervical glands took place, and in her most probably it was strumous rather than syphilitic. The contrast in this respect was very great between the *first* and *second series*, and still greater in respect to the two cases which I record in the present paper (*third* and *fourth series*). In neither of the two latter was the nature of the disease suspected until the skin was covered with secondary rash. In both the chancre on the arm became very large and remained open for several months. In both the eruption came out most copiously and was attended by great loss of flesh and strength. In both iritis of a very severe character occurred. One of them was cured, both as regards local phenomena and general health, by a course of mercury; in the other the disease under inefficient treatment has lingered for twenty months and the patient is still suffering much from its effects. It is of course too

early to obtain data as to the relative liability of the patients to the tertiary forms of syphilis, but so far as the primary and secondary symptoms are concerned I cannot speak too strongly as to the vast apparent advantage of the mercurial plan. The lesson of the cases is very clearly opposed to the too prevalent modern doctrine that it is well to wait for secondary symptoms before beginning specific treatment, and would appear to indicate that the latter should be adopted as soon as ever the condition of the chancre permits of an accurate diagnosis. I may also in passing be permitted to ask attention to the interesting illustration which these cases afford of the manner in which mercury interrupts the evolution of syphilis and delays the occurrence of secondary symptoms. In all the cases which were not treated secondary symptoms showed themselves from the sixth to the ninth week after the inoculation, whilst those treated by mercury did not show symptoms until from five to seven months afterwards.

In conclusion a few words must be said as to the best means by which we may hope to prevent the occurrence of these lamentable accidents in future. Foremost under this head I would put the diffusion of the knowledge amongst the profession that such accidents are possible. Until quite lately almost the whole British profession was incredulous on this point, and in spite of the publicity which was given to the facts two years ago there still remain, I believe, many who are either uninformed or unconvinced. The vaccinator who proceeds in his duties with the fear of syphilis before him can I think incur but little risk in the matter. He will in the first place select his vaccinifer carefully, avoiding all children whose parents are not known to him. He will for the most part avoid all first-born*

* Of the vaccinifers in my four series of cases two were first-born children, the third was the first which had lived, the parents having lost two previous children in early infancy, while in the remaining case, although the child had a brother and sister living, and apparently in good health, there was the fact that the latter had in infancy long suffered from ulceration at the anus. I am bound, however, to admit that if this the second child had been presented as a

children and wait until by the development of one healthy child some guarantee of freedom from taint on the part of the parents has been given. There certainly cannot be any difficulty under ordinary circumstances in procuring vaccinifers who are absolutely free from risk. Next to the scrupulous selection of the child from whom to vaccinate come the obvious precautions of avoiding the use of blood and of recent exudation from the walls of the vesicle, but these and many other matters of detail have already been so well enforced that it is needless to allude to them further.

vaccinifer, there would, as far as can be ascertained by inquiry now, have been no facts likely to arouse the suspicion of the surgeon. The mother would have been able to produce the guarantee of an older child in good health.



A SUCCESSFUL CASE
OF
ABDOMINAL SECTION
FOR
INTUSSUSCEPTION;

WITH REMARKS ON THIS AND OTHER METHODS OF
TREATMENT.

BY
JONATHAN HUTCHINSON, F.R.C.S.,

SENIOR SURGEON TO THE LONDON HOSPITAL; SURGEON TO MOORFIELDS
OPHTHALMIC HOSPITAL AND TO THE HOSPITAL FOR SKIN
DISEASES.

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1874.

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THE case of intussusception which I am about to describe came under my care at the London Hospital in 1871.

The patient was a somewhat delicate female child aged two years. She had previously been seen by my colleague Mr. Waren Tay, who had diagnosed her disease, and by whom she was transferred to my care in order that she might be admitted as an in-patient.

From her anus there protruded a portion of bowel about two inches long, deeply congested and much swollen. By the side of this the finger could be passed, its full length, into the rectum without reaching the point at which the intussusception began. On carefully examining the extremity of the protruded part, I noticed that it did not present merely a rounded opening as usual in such cases. I was able easily to identify the pouch and valve of the cæcum, with the

opening into the ileum. Of these parts it was of course the mucous membrane which was visible, and the appendix cœci was wholly concealed between the folds of the intussusception. This discovery rendered it evident that we had to deal with an involution of bowel of very unusual length, which commencing at the cæcum had allowed the ileum to pass through the entire length of the colon, and actually to become extruded at the anus.

On examination of the child's abdomen externally the tract of bowel involved could be felt like a long firm sausage passing down the left side.

The mother of the child gave us the history that the latter had begun to suffer from pains in the abdomen, rather suddenly, about a month previously. Her first attack of pain was one Sunday afternoon, and was such as to cause screaming. It was quickly followed by a motion which contained blood, and by frequent vomiting. A fortnight after this, the child having been ailing the whole time, a protrusion of bowel was noticed at the anus. This was reduced by the surgeon then in attendance, and a cork pad was fitted over it. It was found impossible, however, to prevent the prolapse from recurring, and the child continued to be sick and to pass blood-stained mucus.

Three days before admission the prolapse increased to such a size that the parents were unable to reduce it, and were obliged on three occasions to call in surgical aid for that purpose. There had been no real obstruction of the bowels, but only temporary constipation at times.

The child, at the time of her admission, looked very ill. Her countenance was pale and anxious, and from her mother's description it was evident that her strength had been failing rapidly during the last few days. It appeared that she was almost constantly engaged in straining to get rid of the bowel which filled the rectum.

Our first measure of treatment consisted in putting the child under chloroform, and then, whilst she was held up by the feet, distending the rectum to the utmost with warm water.

By this means the involuted part could be forced up into the abdomen so as to be quite out of reach of the finger, and once or twice I tried to hope that reduction had been effected. On each occasion, however, when the lower bowel was allowed to empty itself, the intussuscepted part became prolapsed as before, and showed clearly that we had gained nothing.

My experience of several other somewhat similar cases, all of which had resulted in death, after patient and repeated attempts by the injection plan, did not encourage me to expect success in this.

It was very evident, from the child's condition, that unless relief were afforded she would not live long, and I therefore felt justified in telling the parents that although an operation would be, in itself, very dangerous, yet I thought that it afforded the only chance.

They begged me to give the child the chance if I thought it was one, and we accordingly determined to lose no time.

The child having been taken up into the operation theatre, chloroform was again administered, and I then opened the abdomen in the median line below the umbilicus, and to an extent admitting of the easy introduction of two or three fingers. I now very readily drew out, at the wound, the intussuscepted mass, which was about six inches long. I found that the serous surfaces did not adhere, and that there was no difficulty whatever in drawing the intussuscepted part out of that into which it had passed. Just as the reduction was finished the appendix cœci made its appearance, confirming the opinion which had been formed as to the precise part of the bowel involved. The opposed serous surfaces did not present a single flake of lymph, and they were congested in only a moderate degree.

Having completed the reduction, I put the bowel back into the abdomen, and closed the wound with harelip pins and interrupted sutures.

The operation had been an extremely simple one, and had not occupied more than two or three minutes.

The abdomen having been well supported by strapping,

cotton wool, and a flannel bandage, the child was returned to bed.

The after-treatment consisted in the use of milk enemata every three hours, with the occasional addition of five minims of tincture of opium.

No vomiting occurred after the operation. No food whatever was allowed to be taken by the mouth during the next two days. The temperature, on the evening of the operation, was 100.5° , but subsequently fell to 99° , and with the exception of the fifth day, on the evening of which it rose to 101.7° , it never exceeded 100° . Chloroform was administered on two or three occasions to allow of the wound being dressed without the child's screaming. The pins were taken out on the fourth day, that is, seventy-two hours after the operation.

I had felt much anxiety as to the healing of the abdominal wound on account of the thinness of the parietes, but nothing untoward occurred.

The child recovered without having ever showed the slightest symptom of peritonitis, and left the hospital in excellent health about three weeks after the operation.

Nothing but fluid food (milk and beef tea) had been allowed through the greater part of this time from a fear of producing any return of the intussusception. The child was fitted with an abdominal support when discharged, but the scar was sound and strong, and there was no tendency to bulging.

The successful issue of a single case goes but a very little way towards proof that the line of practice adopted was the proper one. I purpose, therefore, with the permission of the Society, to discuss this question in some detail; and the first items of evidence which I will mention are the cases which had previously come under my own observation.

About twelve years ago I operated for harelip upon a delicate child about ten months old. The child had been brought from Somersetshire on purpose to have the operation done, otherwise its feeble state of health would have caused

me to defer it. The lip healed well, but about the tenth day the child began to pass blood and slime. On examination *per anum* I found a long intussusception occupying the rectum. It never protruded at the anus. I tried during six days a great variety of means with a view to the replacement of the involution. Chloroform was repeatedly given. Injections of air and of water were made over and over again, and in various positions of the body, and attempts were also made with a long tube to push the bowel into place. Several times I thought I had succeeded, and on one occasion the passage of a considerable quantity of fæces made me feel confident that reduction had been effected. About six hours after this occurred, however, the child, who had been gradually failing in strength, died. No post-mortem was permitted, and I am unable to state whether the reduction was complete. My colleague, Mr. John Adams, had, on one occasion, seen the patient in consultation with me, and had assisted in attempts at reduction.

A few years later I saw another similar case in consultation with Dr. James and Dr. Bright, at Forest Hill. Our patient was a remarkably fine healthy boy, of about three years of age. A long intussusception occupied his rectum, and came low down, although it never actually protruded. Again we tried every plan that we could think of, but without success. Enemata were used in the most forcible manner with the child's body inverted, and they were repeated several times a day, and often under the influence of chloroform. Bougies of various kinds were also carefully tried.

The child sank from exhaustion about a fortnight after the commencement of the symptoms.

Dr. James obtained a post-mortem which confirmed the diagnosis as to the condition of parts.

A third case was brought under my notice by Mr. Waren Tay three years ago. Its subject was a female child aged about fifteen months. Blood had been noticed escaping from the bowel on the day before Mr. Tay was consulted. The child had great pain and was repeatedly sick. On

examination of the bowel, Mr. Tay discovered at the distance of two or three inches from the anus the extremity of a long intussusception. The portion of bowel involved could also be easily felt through the abdominal wall. Repeated attempts were made by manipulation and by injections to effect replacement of the bowel, but without success, and on the seventh day from the commencement of the symptoms the child died. The autopsy showed an intussusception of the transverse into the descending colon, involving, however, only about two inches of the bowel. It was thought probable that the greater portion had been reduced. The small intestines were much distended, and were somewhat congested on their peritoneal surface, but were quite free from lymph. There were no adhesions whatever of the opposed peritoneal surfaces of the intussuscepted part, and it could have been reduced by traction with the greatest ease.

The particulars of a fourth case, which occurred in a young adult man, have been recorded by me in vol. vii, p. 193, of the 'Pathological Society's Transactions.' In this case the patient lived four months from the commencement of his symptoms, and at the post-mortem six inches of the ileum, the entire cæcum, and first part of the ascending colon, were found invaginated within the latter. The coats of the bowel were much thickened, but there were no adhesions, and reduction by traction from within would have been quite practicable.

My experience does not afford a single case at all to be compared with the preceding, in which the patient survived. I treated successfully, by means of injections under chloroform, a case in which I had conjectured that intussusception was present, but the diagnosis did not rest on any certain data, and the stage was a very early one. The patient was a delicate little boy. He had been sick, and had passed slime and blood. I thought that I could feel through the abdominal wall a lump very much like that caused by an intussusception, but there was nothing to be

felt by the rectum. After a free injection under chloroform the bowels acted and the child recovered.

In another case I had a good deal of trouble with a short intussusception about five inches from the anus, which had resulted from the too rapid reduction of an ordinary prolapse of the rectum, seven or eight inches in length. In this instance, after a considerable manipulation, I was successful in effecting a complete reduction.

Thus, it will be seen that at the time the case which is the subject of this paper came under my care my own experience did not supply a single one at all parallel to it in which the patient had been saved; whilst in three all endeavours had resulted in disappointment. In fact, such had been the impression which these cases had made upon my mind, that I had quite determined to resort to operation when next any similar one should present itself.

The case which I have brought before the Society is, so far as I am aware, the first successful one of its kind in English practice. The operation itself, however, is by no means a novelty, and at least three examples of its successful performance are on record.

I may be permitted briefly to refer to the particulars of these.

In one recorded by Velse, and quoted by M. Hévin, in the 'Memoirs of the Royal Academy of Surgery of Paris,' 1784, the patient was a woman aged 50. Intussusception was diagnosed by Nuck, at whose suggestion the operation was performed. The incision was made on the left side of the abdomen, four fingers' breadth from the umbilicus. The intestine was drawn out, and the intussusception was liberated without difficulty, as no adhesions were encountered. The wound was closed and the patient recovered, and lived for twenty years afterwards. In the performance of this operation the intestines were fomented with tepid milk, and the intussuscepted part was well oiled. It is spoken of as having been very easy of performance.

The next case occurred in 1825, and is recorded by Dr. Fuchsius, of Olpe, in 'Hufeland's Journal' (quoted in the

'Edinburgh Medical and Surgical Journal,' July, 1825. Its subject was a man aged 68, who was seen on the sixth day of his illness. In the neighbourhood of the navel, rather on the right side, there was evident hardening and tenderness, which increased and somewhat changed position during attacks of spasm. After five days of further treatment by clysters, &c., the abdomen was opened. An incision was made on the outer edge of the right rectus two inches above the navel. The intussusception was soon found. There were no adhesions, but such difficulty was encountered in effecting reduction that the surgeon decided to open the intestine. This was done by an incision two inches long, admitting of the introduction of the fingers into the intussuscepted part. Reduction was then accomplished, about two feet of bowel being disengaged. The wound in the intestine was stitched up. The patient recovered. The operator recommends, I have no doubt very judiciously, that in future operations the incision should be made in the linea alba, and that, if it be necessary to put stitches in the intestine, they should be cut close off instead of being left with a long end, to come out at the abdominal wound.

A third case of recovery occurred in the practice of an American surgeon, Dr. Wilson, and is recorded in the 'American Journal of the Medical Sciences' for 1836. The patient was a negro aged 20, and the intussusception had lasted seventeen days. There were adhesions, and great difficulty was encountered.

In British practice the operation appears to have been performed only once, and then under very unfavorable circumstances. The patient was an infant only four months old, in whose case Mr. Spencer Wells was consulted, on the fourth day of an intussusception with acute symptoms. The diagnosis was positive, for the involuted portion of intestine could be reached by the finger in the rectum. It was not till the fifth day, when the patient was almost dying, that the parents of the child consented to the operation. The abdomen was opened in the middle line below the umbilicus. The intussuscepted portion was easily found,

but the constriction was so tight that it was not without great difficulty that it was reduced. Its release was at length accomplished, the intestines returned, and the wound closed. The bowel above the constriction being greatly distended with flatus some needle punctures were made for its relief. The child died about five hours after the operation.

As regards other fatal cases after operation, as already implied, I have not been able to find any in English records. Several continental writers refer vaguely to such, and some speak of them as if they had been numerous. I have found a case reported by Carrier, of which the following are the particulars (as given in 'Virchow's Jahresbericht'). The patient was a man aged 23. Pain came on suddenly, and a tumour could be felt in the ileo-cæcal region. On the fifteenth day the abdomen was opened, and an attempt was made to extricate an intussusception which was discovered, but the attempt was unsuccessful. The small intestine higher up was therefore opened. The patient died seven hours afterwards. The post-mortem showed an intussusception of the ileum into the cæcum.

Fatal cases have also been recorded by Max Hertz, Pirogoff, and Gerson. In two of these great difficulties were encountered in freeing the intussuscepted part, and in Pirogoff's case it was found impracticable.

Before attempting further to discuss the propriety or otherwise of this operation I may suitably refer to the symptoms which characterise intussusception, and to some of its natural terminations.

There is a class of cases, and, perhaps, not a very small one, of which the one I have recorded is an example, in which all obscurity as to diagnosis is removed by the discovery of the intussuscepted bowel in the rectum. In all suspected cases this examination should be made. It is quite evident from the descriptions given of the post-mortems in many cases that had the bowel been sought for by the anus it could have been felt. The symptom next in value,

and, indeed, perhaps not second in real importance, is the manipulation of the abdomen and the discovery of the long or oval sausage-like mass which an intussusception constitutes. This is far more easily done than is generally thought possible, especially so with the aid of chloroform. Unless the parietes of the abdomen be fat my impression is that by firm pressure, the patient being under the full influence of an anæsthetic, all doubt as to the existence or non-existence of intussusception, and as to the completeness or incompleteness of its reduction, may usually be removed.

Amongst the other less important symptoms we must mention pain in the abdomen, attacks of spasms, the passage of bloody mucus or of pure blood by stool, the existence, in some cases, of obstruction of the bowels, and in some of almost constant desire to strain at stool. These symptoms will vary much in degree of severity in different cases, and it is of considerable practical importance to remark that the cases may be roughly grouped, much as we do those of hernia, by reference to the tightness of the constriction. We have cases of intussusception accompanied by *strangulation*, and we have others which are *irreducible* only. The former tend rapidly either to the death of the patient, or his relief by gangrene of the constricted part. Their duration is rarely more than a few days. Those, however, in which there is only an irreducible invagination without either stoppage of the contents of the tube or interruption in its blood-supply may run a prolonged course, and they have a greatly diminished chance of spontaneous cure by gangrene. It is in these latter that operative interference is most necessary and has the fairest chance of success. In these the patient may live on for weeks, and the surgeon is permitted a good opportunity both for establishing his diagnosis and proving the inutility of other measures of treatment. The patient's death when it at length arrives is brought about more by exhaustion from long-continued pain than from any inflammatory process. In this class of cases I believe it would seldom be found that the coats of the intestine had become adherent to each other, or that there was any material

difficulty in effecting reduction after opening the abdomen. If the operation were resorted to in cases of *acute strangulation* there would always be the risk that the surgeon might find the parts in a state of gangrene, and might discover that he had interfered only to take away the patient's last chance.

It seems, therefore, of great importance to insist that before attempting the operation the tightness of the strangulation should be estimated.

The diagnosis between mere irreducibility and tight strangulation will usually be easy. In the one there will be severe sickness, constipation, and great general distress tending to collapse, whilst in the other the bowels will continue to act, sickness will be almost wholly absent, and the patient may suffer comparatively little.

I cannot better illustrate this statement than by reminding the Society that in my own case the state of things had existed for a month, and that so slight had been the patient's general symptoms that a surgeon had ordered a cork pad to keep back what he supposed to be an ordinary prolapse.

The same mistake is mentioned as having occurred in several other cases on record.

If in a case of tight strangulation with severe symptoms the patient were seen early and quite before any indications of collapse had appeared, my impression is that opening the abdomen (insufflation, &c., having failed) would be safer than to leave the case to the chance of cure by gangrene, but if the stage were more advanced I think I should prefer to give opium and trust to nature's method.

Before finally deciding as to the need of surgical interference in that class of cases in which, as I have just shown, it is alike most hopeful and most necessary, we must ask what other chances of recovery are before the patient.

Given a case of intussuscepted bowel without sickness and without constipation, therefore, presumably without strangulation, what degree of probability is there that recovery may be obtained either by natural processes or by methods of treatment short of operation? I have just hinted that the

chances of gangrene are not great.¹ The constriction is not tight enough to cause it, and although it must be granted that in a few instances after the bowel has remained for considerable periods in a state of mere incarceration, gangrene does eventually occur, yet it is a rare event; much more commonly the patient sinks from exhaustion. If the chances of recovery by gangrene be but little my impression is that those by spontaneous return of the parts to their natural condition, or their reduction under treatment by insufflation, &c., are much less. At any rate the surgeon will soon know how much he has to hope in either of these directions. I have not found any case recorded in which spontaneous return of a well-recognised intussusception occurred, and those in which art succeeded are comparatively few. It is, of course, the surgeon's duty to give a patient trial to injections, to use fluids and air alternately, and to use them with the patient's body inverted and with the muscles set at complete rest by an anæsthetic, but if he should not succeed quickly by these means it is not likely that he will succeed at all.

¹ Nor must it be forgotten that even when gangrene occurs it does not necessarily lead to recovery. In several cases in the table appended to this paper death followed the expulsion of the detached portion. Dr. Hilton Fagge, in an excellent paper in the 'Guy's Hospital Reports' for 1869, writes as follows upon this point: "Now, as we have already seen in ileo-cæcal intussusception 'expulsion' comparatively seldom occurs, and when it does occur it frequently only postpones the fatal termination instead of entirely preventing it. The patient dies some months afterwards from contraction of the cicatrix, which had formed at the seat of the disease. This appears to me to afford a weighty additional argument in favour of the attempt to explore and pull out an ileo-cæcal intussusception, when the case is directly diagnosed at an early stage, and when inflation has failed to overcome the disease." The precise cause of death suggested by Dr. Fagge is a very probable one, but there are others yet more frequent. A case under the care of M. Fanchon ended fatally three days after the expulsion, there being an abscess at the seat of disease. A case recorded by Dr. Baillie, in which a yard of colon had been passed, resulted in the death of the patient three weeks afterwards. In two other cases death occurred two and four weeks respectively after the sloughing. In another a post-mortem showed a cavity containing fæces, which intervened between the two ends of the bowel; and in another, fever, vomiting, and diarrhœa, preceded death.

Nearly all the recorded instances of success were very recent cases or cases in which the intussusception was small. They serve but little to encourage the surgeon when he encounters such a case as that which I have just recorded. In very few, indeed, was the intussusception long enough to be felt in the rectum, and in scarcely any did success follow after several failures. The opinion of some of our best authorities is so definite on this point that they recommend that all attempts at replacement should be abandoned if they have not succeeded within a short period.

The literature of intussusception is very large, and it is not my intention to trouble the Society with any attempts at its statistical analysis.

My friend and colleague Mr. Waren Tay has, however, kindly collected for me references to a great number of cases, and from these I may be permitted to extract such facts as may seem to bear most definitely on the subject under discussion. We have confined our attention to cases in which the intussusception occurred in the lower bowel.

In the table appended to this report will be found the particulars of numerous cases in which the intussuscepted part presented into the rectum, and either was or might have been felt by the finger. This table must not be considered in any sense exhaustive, but it may yet furnish us with some valuable data.

Of these cases a very few ended in recovery *without gangrene*. In one of these an infant aged eighteen months, in whom an intussusception could be felt by the finger in the rectum, was treated early under the care of Dr. Steele by powerful injections of warm water, and had no return of the symptoms. In a second, a child, under the care of M. Cabaret, had prolapse of bowel from the anus twelve inches in length; whilst at the same time a sound could be passed up for some distance between the rectal mucous membrane and the invaginated parts. Reduction was effected by a gum elastic bougie, which was retained for several hours to prevent relapse. A similar measure was successful in the hands of Dr. Osborne, in a very similar case. It is to be noted that in

all these three cases it would appear to have been the lower part of the colon only which was involved, and it is obvious that in such the chance of success is far greater than when the cæcum or the small intestine comes down.

In the first of the cases of recovery *after gangrene* the patient was a boy aged 6, in whom the early symptoms had been those of strangulation. The bowel appeared at the anus, and about the eighth day a portion, twenty-three inches, came away. In the second case, again, we have symptoms of severe strangulation, and the patient, a girl aged 11, appeared to be at the point of death. As early as the fifth day a portion of colon, cæcum, and mesentery, measuring nearly fourteen inches, was detached.

In the third case the patient was a man aged 40, who voided twenty-eight inches of colon on the fourteenth day.

The fourth case is one of the most interesting on record, from the unusual length of the period before the bowel separated. The specimen was exhibited by Dr. Quain, before the Pathological Society, and the case is recorded in the tenth volume of its 'Transactions.' The patient, a boy aged 5, had suffered for four months from obscure abdominal symptoms, and was finally relieved by the escape of twelve inches of bowel including the cæcum, part of the ileum and part of the colon. He had had irregular constipation and some sickness, but at times his appetite had been voracious. He had never passed blood.

In the fatal cases the influence of early age in accelerating the event seems well marked, a large majority being infants under the age of one year, who died after periods of from one to three days' illness.

It is clear that if, in infants, operative interference is to be of any use it must be resorted to very early. Examination of the cases in which the patient was under two years of age shows that eleven died within two days, five lived as long as the sixth or seventh day, one to the twentieth, and a single very exceptional one survived for a period of nine weeks. This last case is published by Mr. Sidney Jones in the 'Pathological Society's Transactions.' In it the small intes-

tine had travelled through the entire length of the colon, and protruded at the anus until as much as six inches were visible. The child had free action of the bowels, took the breast well, and never vomited. In the first instance, however, severe symptoms of obstruction had been present. Death was finally caused by exhaustion from straining and by the slowly progressing gangrene of the extruded portion. Mr. Jones mentions in his account of the post-mortem a fact of very great importance in reference to the question of operation—that the serous surfaces of the opposed portions of bowel were adherent along their whole extent by firm, fibrous membrane.

In the absence of any data as to the manner in which operations of this kind are borne by very young children we shall probably be right in believing that they are far less hopeful than in those somewhat older. On the other hand it is our duty to remember that the cure by sphacelus, which occurs with tolerable frequency in others, is scarcely ever met with in infants, and that unless rectification is obtained by injections, without much delay, speedy death is almost certain to result.

Very valuable information might be furnished to the surgeon by post-mortem examination as to the feasibility of operative interference in these cases; unfortunately, however, but few of those who have published cases give us specific details on this point. During the last session of the Pathological Society, Dr. Edwards Crisp exhibited a specimen from a child aged eight weeks, with the statement that so tightly was the invaginated part enclosed that it would have been impossible to withdraw it. Mr. Sidney Jones in one case, as just mentioned, found the peritoneal surfaces universally and firmly adherent. In two cases of my own and in one of Mr. Waren Tay's it was found, at the post-mortem, that traction from within the abdomen easily reduced the invagination, and that there was no material damage to the coats of the bowel. In a very considerable number of published cases the details of the post-mortem warrant the belief that an operation would not have been difficult, since no mention

is made either of tightness of constriction, adhesions or gangrene.¹

One fact disclosed by post-mortem records I may ask especial attention to, and that is the almost uniform absence of peritonitis as a complication. This is specially noted in a great number of cases. In intussusception as in strangulated hernia, and other forms of abdominal obstruction, it may, I think, be taken as an established fact, that unless actual perforation has occurred there will be no peritonitis.

In conclusion, that I may not further weary the Society by the details of isolated facts, I may briefly record my conviction that any one who will carefully examine the evidence for and against will come to the conclusion that operations for the relief of intussusception are not only warrantable, but that in a large number of cases they are urgently demanded.

The cases most hopeful are those in which the symptoms denote incarceration rather than strangulation, and in them the surgeon may take the knife in hand with a good prospect that he will encounter no serious obstacle, and that he will not find either very tight constriction, adhesions, or gangrene. Of the other cases, there are many in which, if the patient be seen early, there is sufficient hope, notwithstanding the severity of the symptoms, to justify the operation, though the surgeon must expect in such to find occasionally that the conditions preclude its completion. Lastly, in a small minority, seen late, or in which the symptoms have from the first been extremely severe, it is probably wisest to

¹ I do not know that we shall gain much by citing the opinions of authors for or against this operation. Amongst many who dissuade us from it are, Dr. Brinton, Mr. Holmes, and Mr. Pollock. On the other side, MM. Rilliet and Barthez, who base their opinion on post-mortem examinations, in which they found reduction very easy, state that "after employing medical treatment during three or four days, and after having made several attempts at inflation, we should not hesitate to perform gastrotomy." Drs. Meigs and Pepper, who quote the above passage, appear to be quite favorably disposed to the operation, and Dr. West's conclusion is to the same effect. Dr. Hilton Fagge, after a careful summing-up of evidence, is a decided advocate of the operation, but suggests it would be well for the surgeon to wait until a case comes before him which is known not to be already of long standing.

decline an operation and to trust to the chance of gangrene.

The following conclusions are appended by way of summary of the facts and statements contained in my paper.

Conclusions.

1. That it is by no means very uncommon for intussusception to begin at the ilio-cæcal valve, and to progress to such a length that the invaginated part is within reach from the anal orifice or even extruded.

2. That it is of great importance in all cases of suspected intussusception to examine carefully by the anus.

3. That in almost all cases of intussusception in children, and probably most in adults, the diagnosis may be made certain by handling the invaginated part through the abdominal wall.

4. That the prognosis of cases of intussusception varies much; first in ratio with the age of the patient; and, secondly, with the tightness of the constriction.

5. That in a large proportion of the cases in which children under one year are the patients, death must be expected within from one to six days from the commencement.

6. That in the fatal cases death is usually caused by shock or by collapse from irritation and not by peritonitis.

7. That in many cases it is easy, by estimating the severity of the symptoms (vomiting, constipation, &c.), to form an opinion as to whether the intestine is strangulated or simply irreducible.

8. That in cases of strangulated intussusception, whilst there is great risk of speedy death, there is also some hope that gangrene may be produced and spontaneous cure result.

9. That in cases in which the intussuscepted part is incarcerated and not strangulated, there is very little hope of the occurrence of gangrene, and it is probable that the patient

will die, after some weeks or months, worn out by irritation and pain.

10. That the chances of successful treatment, whether by the use of bougies or by the injection of air or water, are exceedingly small, excepting in quite recent cases, and that if the surgeon does not succeed by them promptly it is not likely that he will succeed at all.

11. That the cases best suited for operation are those which have persisted for some considerable time, and in which the intestine is only incarcerated, and that these cases are also precisely those least likely to be relieved by any other method.

12. That in the cases just referred to, after failure by injections, bougies, &c., an operation is to be strongly recommended.

13. That the records of post-mortems justify the belief that, in a considerable portion of the cases referred to, the surgeon will encounter no material difficulty in effecting reduction after opening the abdomen.

14. That the circumstances which might cause difficulty are, first, the tightness of the impaction of the parts; secondly, the existence of adhesions; and thirdly, the presence of gangrene.

15. That in selecting cases suitable for operation the surgeon should be guided by the severity of the symptoms, in his estimate of the tightness of the strangulation, and also as to the probability of gangrene having already set in.

16. That in cases in which the patient's symptoms are very severe, or the stage greatly advanced, it may be wiser to decline the operation and trust to the use of opiates.

17. That the operation is best performed by an incision in the median line below the umbilicus.

18. That in cases of intussusception in young infants (under one year of age) the prognosis is very desperate, scarcely any recovering excepting the few in whom injection treatment is immediately successful, whilst a large majority die very quickly.

19. That the fact just referred to may be held to justify,

in the case of young infants, very early resort to the operation.

20. That it is very desirable that all who in the future have the opportunity for post-mortem examination of intussusception cases should give special attention to the question as to whether an operation would have been practicable, and should record their results.

TABULAR STATEMENT

Of the Results of different Plans of Treatment, &c., in Cases of Intussusception of or into the Lower Bowel.

THE following table has been compiled for me by Mr. Waren Tay, and comprises cases more or less closely similar to the one which is the subject of my paper. We have selected from various sources the recorded examples of intussusception of the bowel, *in which the intestine passed low down into the colon*. We did not wish to include cases in which the small intestine alone was involved, since these, both as regards treatment, symptoms, and probable results, belong to a different category. It was necessary, therefore, to adopt some definite line of limitation, and this we have found in the presence or otherwise of the intussuscepted part in the rectum. It is believed that no cases are included in the following list in which the bowel was not either discovered by the finger or, at any rate, might have been, had an efficient examination been made. It will be seen that this discovery of the bowel by the finger is a symptom of the utmost importance, since it places the diagnosis, both of the nature of the lesion and the part of bowel involved, beyond question.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
1	Dr. Trevor, Am. Jour. Med. Sci., Jan., 1852, p. 277	12 years, M.	Occasional pain for two or three weeks, then severe pain followed by vomiting; tumour felt in left side of abdomen and on rectal examination; attempts at replacement with stomach tube, etc.	4 days, death	Intussusception of 12 inches of jejunum into the succeeding 12; the mass thickened by inflammation; there was no general peritonitis; the upper end was just below and within the arch and descending colon, and the lower end was firmly impacted in the pelvis; two smaller ones were found of 2 inches and 1 inch in length	Was diagnosed. This was small intestine, yet forced down nearly to anus.
2	Mr. E. Y. Steele, Lancet, 1849, vol. i, p. 680	8 mos., M.	Passage of blood; tumour felt in rectum; slight prolapse; enemata, etc., were without avail	2 or 3 days? death	No post-mortem obtained.	
3	Do.	4 mos., M.	Tumour felt in the rectum; various efforts at reduction made, but without avail	2 days, death	No general peritonitis; the lower end of the ileum, the cæcum, the ascending and greater part of the transverse colon, were invaginated into the sigmoid flexure; the upper two thirds of the innermost portion were of a claret colour, the lower third greenish brown, and in a state of complete sphacelus.	
4	Do., Lancet, 1859, March 19, p. 287	18 mos., F.	Tumour felt in the rectum; "a considerable length of inverted gut was occupying the rectum;" there was a watery discharge tinged with blood; the case was treated early by power-ful injection of warm water and	6 to 12 hours, recovery	There was no feeling perceptible as if the intussusception had suddenly given way, but the gut did not again bulge down into the rectum	An instance of success by enemata.

1	Mr. Med. Chr., 13, 1858, Schmidt's Jahrbuch., 101, 1859, p. 322	M.	then prolapsus which could be replaced; on seventh day a sud- den prolapse, which could not be reduced; on the eighth day twelve inches of colon were pro- lapsed, while a sound could be passed a long way upwards by the side of it	recovery	fingers only were without avail; Le Pelletier's method was suc- cessful; pressure with a gum- elastic sound, which was not re- moved finally till the third day, as the prolapse recurred a few minutes after the pressure was withdrawn	probably the pro- lapse was of rectum chiefly. Compare with the first men- tioned, on page 39.
6	Dr. Worthington, Am. Journ. Med. Sciences, Jan., 1849, p. 97	34 years	For two years had had various intestinal symptoms; for six weeks had diarrhoea and protru- sion of bowel at the anus	6 weeks? death	The cæcum was invaginated along the whole length of the colon and rectum, carrying with it the lower portion of the ileum and the first part of the colon; more than 2 feet of bowel had been inverted; the cæcum must have passed through the sphinc- ter in the child's efforts to evacuate the bowels.	
7	Mr. T. Blizard, Med. Chir. Trans., vol. i, p. 169, 1812	5 mos., M.	A tumour about the size of an egg on the left side of the abdo- men	4 days, death	The lower end of the ileum, the cæcum and its appendix, the ascending colon, &c., were in- vaginated into the sigmoid flex- ure and rectum to within 1½ inch of the anus; there was no peri- tonitis; the invaginated parts were gangrenous; "they might have separated had the child's constitution not given way."	
8	Mr. Langstaff, Edin. Med. Journ., July, 1807, p. 263	3 mos., ?	Vomiting; passage of blood and mucus; hard tumour felt on the left side of the abdomen; pro- lapsus	5 days, death	The end of ileum, the cæcum, and colon invaginated into rec- tum; there was also a smaller invagination in the opposite di- rection.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
9	Penquier, L'Union Médicale, Aug. 22, 1861; Dr. Smith, Amer. Journ. Med. Sci., Jan., 1862	4 mos., M.	Passage of blood; tumour felt five or six inches from the anus, and a hard elongated tumour in the left iliac fossa	2 days, death	Cæcum and its appendix, ascending and transverse colon into descending colon, close to rectum. It is stated that invagination of the large intestine is common among children in Brittany, where the practice prevails of bandaging them tightly in linen and leaving them alone for some hours.	
10	Dr. Carter, Lancet, June 9, 1849, p. 607	4 mos., F.	A large sausage-like body felt in left iliac region; enemata only reached a certain point	6 days, death	Invagination found in left iliac region, but not described	Probably could have been felt per anum.
11	Mr. Nind, Lancet, 1849, vol. i, p. 681	4 mos., M.	One sanguineous stool	4 days, death	Cæcum and ascending colon in sigmoid flexure. "The invagination was so complete that from the congestion, &c., which had occurred I could not reduce it till the enclosing portion of the gut was divided nearly in its whole length;" it "was of a deep purplish colour, with small ash-coloured patches of gangrene."	Ditto.
12	Dr. Edwards, Med. Times & Gaz., 1861, vol. ii, p. 531	3½ years, M.	Pain and swelling of right side of abdomen, which passed away eight months previously; prolapse, which could be returned four months previously; for two days the prolapse became perma-	4 days? death	<i>Post-mortem</i> .—A portion of bowel 2½ inches in length protruded from the anus; a large mass could be felt in the left side of the abdomen; the cæcum and colon invaginated into descending colon, sigmoid flexure and	

13	Mr. Ash, Med. Times & Gaz., 1867, vol. ii, p. 505; Brit. Med. Journ., 1868, vol. i, p. 117	6 years, F.	A tumour could be felt within the anus, and finally a prolapse occurred; attempts to push the tumour back unsuccessful	10 days, death	inverted cæcum and its appendix, and the opening of the ileo-cæcal valve was visible on the prolapsed part. A post-mortem was obtained, but no description of the state of parts is given; it is called an intussusception of the rectum.
14	Mr. Young, Brit. Med. Journ., vol. ii, p. 779, 1859	9 mos., M.	A tumour could be seen and felt less than an inch from the anus	28 hours, death	<i>Post-mortem.</i>—Ileum, cæcum, and colon invaginated into colon and rectum.
15	Dr. Philipson, Brit. Med. Journ., Sept. 24, 1864	10 mos., M.	Tumour felt about four inches from the anus; repeated attempts at reduction (digital, enema, and insufflation) unsuccessful	28 hours, death	Ileum, cæcum, and colon, invaginated into colon and rectum, being the part near the ileo-cæcal valve. "An attempt was made to reduce the tumour in the same manner as during life, but was quite unsuccessful. Traction was then made on the small intestine, and it was only dislodged after a considerable amount of force." "The cæcum and appendix were then drawn out, but still more force was required; the adhesions were very firm."
16	Dr. Merriman, Lancet, 1844, vol. ii, p. 298	Child	Not given	4 days, death	Cæcum, appendix, and ileo-cæcal valve invaginated into colon Probably might have been felt if examination per anum had been made during a straining fit.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
17	Mr. Snow, <i>ibid.</i>	Child	Not given	Death	Ileum into sigmoid flexure	Same as No. 16.
18	Markwick, <i>Lancet</i> , 1846, vol. ii	4 mos.	Blood passed; the symptoms began soon after birth	4 mos.? death	The colon was intussuscepted into the sigmoid flexure; it was impossible to withdraw it	Ditto.
19	Clark, <i>Lancet</i> , 1849, vol. ii, p. 206	6 mos.	Left side of abdomen hard and prominent	2 days, death	Ileum, cæcum, and colon intussuscepted into descending colon	Ditto.
20	Burford, <i>Lancet</i> , Oct. 31, 1840	6 mos.	Passage of blood	3 days, death	<i>Post-mortem.</i> — "Was surprised to find the rectum fall." Ileum, cæcum, and colon invaginated into sigmoid flexure and rectum.	
21	M. Robin. M. 3½ years Hévin's <i>Mémoire</i> in <i>Mém. de l'Acad. Roy. de Chir.</i> , t. xi, p. 324	3½ years	Prolapsus after suffering more or less for three months	6 days, death	Cæcum and greater part of colon in the rectum. "It was found impossible to withdraw the invaginated intestine, it had contracted strong adhesions."	
22	M. le Blanc. M. Sabatier's <i>Mémoire</i> (<i>Mém. de l'Acad. Roy. de Chir.</i> , t. xv, p. 35)	A child	Prolapse six or seven inches in length	15 days, death	Ileum, cæcum, and colon, were invaginated into the rectum; it was impossible to reduce this.	
23	M. Puy, quoted by M. Sabatier (<i>l. c.</i>)	40 years, M.	Prolapse to the extent of about six inches; the first attack occurred about two months before death, but he apparently re-covered from this	16 hours, death	Ileum and colon invaginated into the rectum.	

25	Lavernet, quoted Dict. des Sciences Med., vol. xxiii, p. 560	9 mos., M.	Discharge of blood; tumour felt in the left iliac fossa	60 hours, death	Ileum, cæcum, and colon in- vaginates into colon and rectum.	colon was invaginated into the rectum to the extent of 13 inches.
26	Monro, sen., Morbid Anat. Ali- mentary Canal; Dr. Smith, Am. Journ. Med. Sci., Jan., 1862	4 mos., M.	Slime and blood discharged	68 hours, death	Ileum and colon into colon and rectum.	
27	Mr. Clarke, Lan- cet, Feb. 10, 1838 (Gorham, Guy's Hosp. Rep., Oct., 1838, p. 331)	11 mos.	Hæmorrhage	62 hours, death	Ileum, cæcum, and colon into colon, close to sigmoid flexure	? Could have been felt.
28	Dr. Baer, Am. Jour. Med. Sci. (Gorham, l. c.)	16 mos.	Hæmorrhage, and large tumour felt in left iliac region	48 hours, death	Ileum, cæcum, and colon into sigmoid flexure.	
29	Mr. Cunningham, Med. Gaz. Sept. 15, 1838 (Gorham, l. c.)	9 mos.	Hæmorrhage, and tumour felt in left iliac region	40 hours, death	Ileum, cæcum, and colon in sigmoid flexure and rectum.	
30	Mr. Whately, Phil. Trans., vol. lxxvi, p. 305	M.	"The valve of the colon at last got as low as the anus, and when he went to stool he only emptied the ileum"	Death	Ileum, cæcum, and colon in sigmoid flexure and rectum.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
31	Dr. O. Ferral, Dub. Path. Soc., Lond. Med. Times, Jan. 16, 1847 (Dr. Smith, l. c.)	12 mos., M.	Intense pain, passage of mucus and blood	6 days, death	Ileum and cæcum into colon; there were two orifices, one leading into the ileo-cæcal valve, the other into the appendix	Probably could have been felt.
32	W. S. Partridge, Prov. Med. and Surg. Journ., May 3, 1848 (Dr. Smith, l. c.)	4 years	Passage of blood and mucus	3 days, death	Ileum, cæcum, and colon invaginated into rectum	Ditto.
33	Dr. Harland, Med. and Phys. Res., Philad., p. 565 (Dr. Smith, l. c.)	5 mos., F.	Prolapse	Not stated, death	Ileum, cæcum, and colon into colon and rectum; the cæcum protruded from the anus.	
34	Mr. Davies, Med. Repos., Dec., 1824 (Dr. Smith, l. c.)	6 years, F.	Diarrhœa, passage of mucus and blood	8 months, death	Ileum, cæcum, and colon into colon and rectum	Probably could have been felt.
35	Dr. Kennedy, Dub. Journ. Med. Science, March 4, 1844 (Dr. Smith, l. c.)	4 mos.	Passage of blood	1 day, death	Ileum, cæcum, and colon were invaginated	Ditto.
36	Mr. Perrin, Lancet, March 26, 1853 (Dr. Smith, l. c.)	3 mos.	Bloody stools	2 days, death	Ileum, cæcum, and colon, in descending colon and sigmoid flexure	Ditto.
37	Dr. Smith (l. c., Case 34)	3 mos., M.	Passage of mucus and blood; prolapse twelve hours before death; distension in right iliac	7 days, death	Ileum, cæcum, and colon into colon, &c.	

39	Journ. Med. Sci., October, 1827 (Dr. Smith, l. c.)	—	—	evacuation; prolapse after seven or eight days, and the next day 23 inches separated	recovery	An invagination was found in the rectum.	sloughing.
40	Jacobi (Dr. Smith, l. c.)	—	—	Tumour felt two or three inches from anus	Death	The invagination began just below the upper part of the sig- moid flexure.	
41	Monro, Edin. Phys. and Lit. Essays, vol. ii, p. 386	18 mos., M.	—	Prolapse; after reduction a tumour could be detected with an opening at the lower part like an os tinæ; large enemata were given, and a long probe of whale- bone armed with sponge was used, but without success	A few days, death	A surgeon gently pulled at the intestine, and a yard and three inches came away; it proved to be a portion of ileum.	
42	Mr. Stanley, Lan- cet, March 11, 1826, p. 813	Middle age, F.	—	Prolapsus	Death	The lower part of the colon and the upper part of the rectum were invaginated into the rectum. He regrets "that this was not felt during life."	
43	Mr. Howship, Ed. Med. & Surg. Journ., April, 1812	4 mos., F.	—	Passage of blood; injections failed	6 days, death	Portion of colon, cæcum, and mesentery, measuring 19½ inches, passed by stool	? Could probably have been felt. Re- covery by gan- grene.
44	Mr. J. W. Bow- man, ib., Oct. 1813	11 years, F.	—	Obstruction of the bowels and every symptom of approaching dissolution	5 days, recovery	—	Ditto, ditto.
45	Mr. Valentine, ib., April, 1826	40 years, M.	—	Passed 28 inches of colon	14 days, recovery	The lower part of the ileum had passed along the whole length of the colon and out at the anus; it was still pervious; there were evi- dences of progressive sloughing.	
46	Mr. Sydney Jones, Path. Tr., vol. viii, p. 179	4 mos., M.	—	Passage of mucus and blood; improvement for three weeks; at the end of forty-six days, pro- lapse; this increased till as much as 6 inches protruded	9 weeks, death		

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
46	Mr. Holmes, Path. Trans., vol. viii, p. 177	40 years, M.	Tumour felt at a distance of about half an inch from the anus	10 days? death	The sigmoid flexure of the colon and the upper part of the rectum were invaginated. In the middle of the ascending colon was a ragged opening, from which fæces had escaped.	
47	Mr. Ballard, ib., p. 185	6 mos., F.	Blood-stained fluid passed per anum	4 days, death	Ileum, cæcum, and colon invaginated into rectum within two inches of the anus.	
48	Dr. Quain, ib., vol. x, p. 160	5 years, M.	Pain in region of bladder; no passage of blood; sickness and constipation	4 mos., recovery	8 inches of the ileum, the cæcum, and 4 inches of the colon were passed by the anus	? Could probably have been felt. Recovery by sloughing.
49	Dr. Buchanan, ib., p. 171	7 mos., F.	Passage of blood; physical examination of the abdomen negative	53 hours, death	Ileum, cæcum, and colon invaginated into sigmoid flexure and rectum to within half an inch of the anus. The intestine could be withdrawn without special difficulty	Stress is laid on the importance of an anal examination.
50	Mr. Nunneley, ib., vol. xi, p. 109	3 years, M.	Passage of a little bloody mucus	11 days, death	A foot of ileum had passed through the ileo-cæcal valve into the large intestine; there was no appearance of inflammation whatever, neither lymph nor blood was effused	? Could have been felt.

52	Trans., vol. lxxvi, p. 305	F.	child not seen for three weeks	death	duced into the anus detected a round substance in the rectum, with an opening in the middle, not unlike an os-tineæ; the finger passed completely round this between it and the wall of the rectum; the enclosed intestine was in a state of commencing gangrene, but could be easily withdrawn; a portion of ileum contained was uninvolved, so was the appendix cæci.	Recovery after reduction by a bougie.
	Dr. Osborne, Aitken's Medicine, vol. ii, p. 814	A child	Tumour felt in the rectum; at the end of thirty-four hours it almost presented at the anus; an elastic bougie was passed into the orifice and pushed up; it carried the intestine with it, but "more owing to straightening of the canal than any force used"	2 days ? recovery	—	
53	Dr. Thomson, Edin. Med. Journ., pp. 300 and 316. From an Italian source	40 years, F.	After seven or eight days colic symptoms; passage of part of colon, the cæcum and its appendix; a month later there were still colicky symptoms, and a hard, circumscribed tumour could be felt in the left iliac region	50 days, death	When the omentum was raised, two openings were found in the colon, one of which received the ileum and its mesentery; the omentum performed the part of an outer coat, so that no fæces escaped from the intestine. The rectum "appeared full of fæces, but on being cut up the ileum and mesentery, for a Parisian foot, were found pushed into the colon as far as the rectum."	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
54	Dr. Greig, Edin. Med. Journ., Oct., 1862, p. 312	4 mos., M.	Passage of blood; tumour in left side of abdomen; an enema could not be thrown up	42 hours, death	Ileum, cæcum, and colon invaginated into rectum to within an inch of the anus. "After removal of the tumour the cæcum was easily drawn out of the colon, and restored to its natural position, but the greatest difficulty was experienced in getting the swollen, small intestine reduced through the ileo-cæcal valve, which seemed even then to be in a spasmodic condition"	Dr. Greig narrates four other cases which recovered under the use of inflation, but in these the tumour was on the right side, probably short intussusceptions. In one of them he had to inflate several times.
55	Dr. Greene, Brit. Med. Journ., March 18, 1871, p. 278	4½ mos.	A tumour, much resembling a small sausage in shape and density, was felt on left side of abdomen; passage of slime	Several days, death	Ileum, cæcum, and colon into descending colon and sigmoid flexure.	
56	Dr. J. St. C. Gray	5 years, M.	Tenesmus, passage of blood at first, then of blood-stained mucus. "There was considerable tympanites, but nothing was ascertained tending to throw light on the case, either by percussion or by examination of the rectum by the finger." Turpentine enemata, then various remedies	8 days, death	There was no trace of peritonitis; colon was invaginated into colon and upper part of rectum; the cæcum was not involved	The distance from the anus is not stated, but possibly by pressing on the abdomen, and examining during straining, the finger might have reached the "upper part of the rectum."
57	Mr. King, Lancet, June 17, 1854	6 years, M.	Suddenly seized with symptoms of ileus; in four days convulsions and insensibility	11 days, recovery	The cæcum and appendix, with part of the ascending colon, passed per anum; afterwards the right lower extremity became swollen and gangrenous, the leg separated	Recovery after gangrene.

59	Ztg. n. F., 1, 31, 1858. Schmidt's Jahrb., 101, 1859, p. 321	M.	vomit and by anus, constipation, and pain; clysters of no avail; petechiæ on various parts of the body; great prostration. On the 9th day some improvement. On the 12th day fæces passed. 13th day the man drew attention to a mass protruding from his anus	recovery	appendix were passed per anum; the gut was in sufficiently good condition for the longitudinal bands, &c., to be made out; the man was a mere skeleton at the time the sphacelation occurred, and though he immediately began to improve, it was six months before he resumed his duties as a soldier	grene.
	M. Sobaux. (M. Hévin, Mém. de l'Acad. Roy. de Chir., t. xi, 1784)	Adult, M.	A portion of colon, 23 inches in length, passed per anum after about three weeks' illness	3 weeks, recovery	—	Ditto.
60	M. Fauchon. (M. Hévin, l. c.)	48 years, M.	The whole of the cæcum, with 6 inches of the colon, and the same length of ileum, were passed after twenty-five days' illness; the patient seemed well afterwards, but died three days later	28 days, death	An abscess communicating with the gut was found	Death after gangrene and separation of gut.
61	Dr. Thomson, Edin. Med. Journ., 1835, p. 301	40 years, M.	The man was thrown down and trampled on; after some weeks he suddenly felt something in the rectum; at last protrusion occurred, and when he laid hold of the mass extruded he pulled away intestine	Some weeks, recovery	The portion of intestine was a span in length, and at one end was found the ileo-cæcal valve	Recovery by gangrene.
62	Dr. Thomson, l. c., p. 304	11 years, F.	Very severe abdominal symptoms; threatened dissolution	5 days, recovery	A portion of the colon, the cæcum and mæso-colon measuring 13¼ inches, was passed by the anus	Ditto.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
63	Dr. Thomson, l. c., p. 305	24 years, M.	Around the umbilicus was an oval swelling larger and longer than a turkey's egg	40 days, recovery	The whole of the cæcum with its appendix was discharged per anum	Recovery by gangrene.
64	Dr. Thomson, l. c., p. 308; also 1836, p. 374	4½ years, M.	Colicky pains in the belly, with passage of blood; protrusion of intestine from anus five days before its separation	Some days, recovery	The boy, while suffering from smallpox, passed the whole of the cæcum with its appendix, and part of the ileum	Ditto.
65	Dr. Baillie, Trans. Soc. Improvement of Med. Knowledge, vol. ii, p. 144. (Dr. Thomson, l. c., p. 312)	50 years, F.	Frequent passage of blood and vomiting; diarrhoea came on and lasted for many days	"Many days," death	No autopsy. A yard of colon was passed three weeks before death	Death, although the gangrenous bowel had passed.
66	Dr. Thomson, l. c., p. 313	30 years, F.	Colic, fever, stercoraceous vomiting. On the 8th day the cæcum and a part of the colon were passed	Death	She died a fortnight after the separation of the bowel in connection with a confinement of a stillborn child; the whole abdomen was filled with purulent serum	Ditto.
67	Dr. Thomson, l. c., 1836, p. 378	40 years, F.	After various abdominal symptoms, passage of a membranous substance	Recovery	It was considered to be the cæcum which was passed	Recovery by gangrene.
68	Dr. Thomson, l. c., p. 378	35 years, M.	Dysenteric symptoms; about 18 inches of colon passed	15 days, recovery	—	Ditto.
69	Dr. Thomson, l. c., p. 380. From Meckel	17 years, F.	"A fever, attended at first with constipation, and then with diarrhoea"	4 weeks, death	Separation of cæcum and appendix, and later, of whole of transverse and ascending colon with portion of ileum, thirteen	Death long after gangrenous detachment of the bowel.

70	Dr. Thomson, l. c. p. 380	7 years, F.	Fever and pains in the abdomen; in the region of the loins was felt a pretty hard swelling of the size of a goose's egg; at length the whole caecum with its vermiform appendix was discharged per anum	Death	At first the patient improved after passage of the intestine, but was at last carried off by fever, colic, vomiting, and diarrhoea. The caecum and part of the ileum had evidently been invaginated into the colon, and had sloughed off	The cause of death is not stated. Death occurred after gangrenous separation.
71	Hill, Month. Jour. Med. Sci., vol. v, 1845, p. 572. (Dr. Peacock, Path. Tr., xv, p. 122)	65 years, F.	Constipation, vomiting, diarrhoea; constipation for a week; then in five days passage of <i>forty-four</i> inches of intestine; forty days later she sank, exhausted	53 days, death	The portion passed proved to have been the sigmoid flexure; only fourteen inches of colon remained and terminated in a cavity containing faeces from which the rectum arose.	
72	Dr. Peacock, l. c. p. 122	67 years, F.	Pain followed by diarrhoea; a large portion of the rectum and colon was passed on the fifteenth day	15 days, recovery	She ultimately recovered, but suffered from pain in the abdomen.	
73	Dr. Hunter (Jedburgh), Lancet, Mar. 9, 1872	9 mos.	Was not very fretful nor peevish; took the breast well till near the end; was very thirsty, and drank eagerly of water; vomiting was almost constant, though it was not faecal at any time. For the last fortnight the ileo-caecal valve protruded at the anus often to more than an inch beyond, and at other times lay up in the rectum when put up	20 days, death	The invagination had begun at the ileo-caecal valve.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
74	Wilson, Clinical Record, Feb., 1870. (Virchow's Jahrb., 1870, Bd. ii, Abth. 3)	4 mos.	—	7 days, death	Ileum, cæcum, and colon invaginated into the colon and upper part of the rectum.	
75	Wagner, Jahrb. f. Kinderh., n. f. iii, 343	2 years, M.	The tumour was felt on rectal examination; after inflation the tumour at once disappeared	Recovery	The child remained well at the end of eighteen months	Recovery after inflation.
76	Ditto	4 years, M.	Tumour on the left side of the abdomen; none felt on examination per anum	Recovery	Clysters had no effect. After eight pumpings with bellows a loud report was heard; the next day there were signs of the tumour returning, but the whole disappeared after inflation. The child remained well	Ditto.
77	Dr. S. Wilks, Lancet, May 21, 1870	6 mos., M.	Sickness; blood by rectum; lump felt to left, above umbilicus, which hardened on pressure. On passing the finger into the rectum a round projection could be felt four inches up with a circular orifice in the centre; inflation was used and the tumour disappeared	24 hours, recovery	"Ileum into cæcum probably." The child remained well for about a fortnight, was then brought with a lump again to be felt and had passed blood. The mother declined further treatment, and the case was lost sight of	The result remained doubtful.
78	Dr. Hilton Fagge, Guy's Hosp. Rep., 1869, p. 289	5 years, M.	Pain and abdominal tumour the only symptoms for two (? four) months; symptoms of strangulation with hæmorrhage four days before death. Peculiar feeling of hardness whenever	4 months, death	The ileum, cæcum, and colon into descending and sigmoid colon. Shreds of lymph (adhesions) of no very recent formation united the parts together; no ulceration nor gangrene: the finger could	

79	Ditto, p. 302	1 year	Had passed blood; twelve inches of intestine (ileum, cæcum, and colon) sloughed	Recovery	—	Recovery by sloughing.
80	K. v. Mosengeil, Arch. f. Klin. Chir., xii, p. 75	7 mos., M.	Doubtful symptoms for fourteen days; then for four days constipation, straining, and propulsion of a tumour nearly to the anus; next day prolapsus; this increased on following day, and the whole was replaced by a catheter. The child appeared well. In a few hours the protrusion returned and no efforts were successful in reducing it. Three days later some mucous membrane sloughed; the next day an artificial anus was formed in the left groin, and an intussuscepted portion of gut found in the portion opened. A catheter could be passed a long way up by the side of the invaginated portion. On the third day afterwards the child died	27 days, death	At first, injections of water and the use of a sound did not succeed, then the latter replaced the intussusception; but later in the same day nothing was of any avail. No post-mortem. The operation performed was of no avail. The operator recommends that in future the incision should be made on the right side. He would have done so here afterwards, but the child was too exhausted	The question of performing abdominal section and replacing the intestine does not seem to have been entertained. It would appear to have been a favorable case. The case is exceptional in respect to the long survival of a very young child.
81	Dr. Steffan. (Dr. Pilz, Jahrb. f. Kinderheilk., n. f., iii, p. 6, 1870)	3 mos., M.	Well-marked symptoms	4 days, death	Post-mortem.—Ileum, cæcum, and colon into descending colon; the folds were covered with blood-stained mucus and what seemed to be layers of fibrine	Cited on account of condition at post-mortem.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
82	Dr. Ecker (ibid.)	6 years, M.	Well-marked symptoms; passage of portion of small intestine; yet, on the fourteenth day, sudden symptoms of peritonitis	Death	No post-mortem	Cited as fatal result after splenecelus.
83	Dr. O. Groos. Berlin. Klin. Woch., 1870, p. 395. (Central. Jahrb. f. Päd., 1871, Bd. ii, p. 58.)	6 mos., M.	After twenty-four hours tumour in left abdomen and in rectum; third day prolapsus; attempts with finger and with clysters without success; repeated second day	5 days, death	<i>Post-mortem.</i> —Peritonitis. Old adhesions of parts of ileo-colic intussusception to each other. The author remarks that the intussusception must have been of old standing, and that the symptoms came on when the passage previously existing was blocked by the subsequent invagination of small intestine into it.	
84	Dr. Max. Herz (l. c. 1872, Bd. i, p. 5)	7 mos., F.	Sickness and constipation; clysters thrown up on third and fourth days; on fifth day blood and slime passed; no tumour; injection of air; sixth day tumour left side of abdomen and in rectum; on the seventh day convulsions	6 days, death	No post-mortem. See also case of abdominal section.	
85	Dr. Faber, Wurtemb. Correspond. Blatt, No. 25 (Vinchow's Jahrb., 1870, Bd.	11 years, M.	Tumour in abdomen just under navel; passage of blood. The tumour was long and tolerably hard	Recovery	After four injections of cold water at different times the tumour disappeared	? Examined by rectum.

87	Jahrb., DL. VII, 1848, p. 59 (No. 42, Pilz*) (Jahrb. f. Kinderh., n. f., Bd. iii, 1870)	M. 8 mos., M.	Seen on fourth day; injections of water of no use; fifth day, air no use; intussusception de- scending; sixth day, tumour felt in the rectum; after seven injec- tions of water the tumours dis- appeared	5 days, recovery	On the fifth day calomel was given in two-grain doses every half hour, <i>i. e.</i> ten grains al- together. The author remarks that his case was interesting, because "in spite of the five days' duration, no adhesion of the sur- faces had occurred"	In this case per- severance with water injections succeeded after insufflation had failed.	Schmidt's Jahrb. seems doubtful.
88	Nissen, Ficke and Opp. Zeitschr., Bd. xix, p. 162 (78 Pilz) *	9 mos., F.	Tumour on left side of abdo- men; prolapsus; reposition with a sound and injections of water	9 days, recovery			
89	Neumann, Inaug. Diss. Halte, 1842 (79 Pilz)*	9 mos., M.	Colon into descending colon; tumour felt by the rectum; sound used	3 days, recovery			
90	Legoupil, Gerson Magazin. (120 Pilz) *	4½ years, M.	Ileo-colic; prolapse; blood passed	30 days, recovery			
91	Prestat, Journ. f. Kinderkrank., 1863, Bd. xli, p. 310	9 years, F.	Symptoms of intussusception, then prolapse, which was re- placed, but the child soon after- wards passed a mass of intestine	7 days, recovery		Sphacelus on eighth day; the lower end of the small intestine with a fold of mesentery. The child was under observation for a year afterwards.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
92	Gieffers, Caspar's Woch., 1815 (156 Pilz)*	13 years	Prolapsus	Recovery	Sphacelus of twelve inches and a half of large intestine	Recovery by gangrene.
93	Van Nes, Schmidt's Jahrb., 1848, Bd. lvii, p. 59	9 mos., M.	Tumour felt in rectum and in abdomen. "After three days symptoms of peritonitis,"	3 to 4 days, death	Transverse colon into descending colon and rectum. After death he opened the abdomen on the left side, in front, and then the intestine. He found a long invagination and could not reduce it; he also found evidences of peritonitis.	
94	Dr. H. A. Beach, Boston Med. and Surg. Journ., Nov. 5, 1868	24 years, M.	Tumour felt in rectum after injections had been given and the lower part of the rectum cleaned out	85 hours, recovery	He placed the man "on his shoulders and knees" and gave injections. The intussusception slipped up, but returned when the man moved about four hours afterwards. It was again driven up	Recovery by injective treatment.
95	W. Pepper, Phil. Med. Times, Sept. 1, 1871 (Virchow's Jahresb., 1871, Bd. ii, p. 152)	6 mos.	—	4 days, death	Invagination of ileum, caecum, and colon into colon.	
96	Cooke, New York Med. Record, May 1, 1871	An adult ?	Constipation; passage of slime for three or four days, then an intussusception detected in the rectum	5 days, recovery	The tumour was pushed up as far as possible, then driven still farther by injections while the patient was on knees and elbows. The next day the injection was repeated and an evacuation followed	Recovery by injections.

98	Gaetano Moretti, Annali Univ. di Med. (Giugno (Virchow's Jahresb., 1871, Bd. ii, p. 153)	40 years, M.	Symptoms of invagination; prolapse of sixteen inches of intestine; it could easily be pushed back a certain distance, but no farther; finally, it came away altogether	Recovery	The portion of intestine which came away was sixteen inches long, and was believed to belong to the sigmoid flexure of the colon. The patient remained under observation for two months, and was then quite well	Recovery by gangrene.
99	Dr. Hodges, Boston Med. and Surg. Journ., Aug. 6, 1868, p. 5	3 years	After various symptoms of colic, vomiting came on, and tumour felt in right iliac fossa; none in rectum. Two days later blood passed, and the next day a tumour was found in the rectum. On the following day the child was sent to hospital. Tumour felt in rectum, and made visible by the use of a speculum. Child in a moribund condition. A steel sound was used, and the tumour pushed out of sight, but the child died eight hours later	4 days? death	<i>Post-mortem</i> .—The cæcum was found to present. No statement as to peritonitis	Tumour made visible by use of a speculum.
100	Hachmann, Zeit. f. gesamt. Med. v. Fricke u. Oppen., Bd. xiv, p. 289 (No. 1 Pilz)	11 weeks, M.	Passage of blood; tumour in abdomen and in rectum	5 days, death	Ileum, cæcum, and colon into colon as far as the rectum.	

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
101	L. Smith, N. York Path. Soc., 1861 (No. 8, Pilz)	3 mos., M.	Passage of blood; tumour in abdomen and in rectum	8 days, death	Ileum through the colon.	See No. 121.
102	Plath, Caspar's Woch., 1839, p. 432 (No. 10, Pilz)	16 weeks, M.	Passage of blood; tumour in abdomen and in rectum	2 days, death	Cæcum and colon into descending colon and rectum; there was no gangrene. The intussusception, it is said, could not be drawn out, owing to constriction at its commencement.	
103	Basedon, Siebold Journ. f. Geburtsk., Bd. vii, p. 512 (No. 26, Pilz)	4 mos.	Passage of blood; tumour in rectum	2 days, death	Colon ascends in colon descendens.	
104	Dr. Staug, Journ. fur Kinderkrank., 1863, Bd. ii, p. 130	5½ mos., M.	Passage of blood-stained mucus; tumour could be felt in the abdomen, and could be seen through the distended anus; a sound was used and clysters were thrown up without effect	4 days, death	Cæcum and colon into colon (transverse). There was a plastic exudation on the layers of intestine in the intussusception, but it did not amount to much; the layers were cut open.	
105	Schwarzwelder, Gaz. Hebdom., 1857, p. 583 (No. 73, Pilz)	7 mos., F.	Blood-stained mucus passed; prolapse; tumour in abdomen noticed	6 days, death	Ileum, cæcum, and colon into colon and rectum; no peritonitis. (Quoted in Gaz. Hebdom. from the Cincinnati Med. Observer, July, 1857, p. 295; case under the care of Mr. Wilson.)	
106	Husch, Caspar's Woch., 1838, p. 617 (No. 81, Pilz)	9 mos., F.	Blood passed; prolapse; the cæcum was outside the anus	30 hours, ?	Ileum, cæcum, and colon into rectum. There were no adhesions nor any exudation.	

		the left side of the abdomen			
108	1861, vol. ii, p. 160 Mr. Matthias Rowe, Lond. Med. Gaz., vol. xv, October 25, 1834	10 mos.	Passage of blood-stained mucus; tumour felt in rectum	30 hours, death	no softening, nor gangrenous ap- pearance of any kind. The ileum, cæcum, and colon in the rectum. A drawing of the parts is given. "It was with difficulty that the colon could be drawn out of the rectum."
109	Augustin, Diss. Inaug. Halle, 1836 (No. 103, Pilz)	2 years, M.	Blood-stained mucus; tumour in abdomen and in rectum	11 days, death	Colon transverse, and descend- ing in the rectum.
110	Abercrombie, Dis- eases of the Stomach, p. 123	2 years and 5 mos., M.	Vomiting, pain, passage of bloody mucus and blood; prolapse on second day; the caput coli protruded	2 days, death	Ileum, cæcum, and colon into colon. There was congestion, no adhesions; the ileum was toler- ably healthy.
111	Neumann, Diss. Inaug. Halle, 1842 (No. 109, Pilz)	3 years, M.	Blood passed; prolapse; tu- mour in abdomen	5 days, death	Colon descending and part of rectum into rectum.
112	Abercrombie, l. c., p. 124	4 years, M.	"It exactly resembled the pre- vious one, except that it was not so extensive"	5 or 6 days, death	Colon descending and part of rectum into rectum.
113	Neumann, l. c. (No. 130, Pilz)	6 years, M.	Blood passed; no tumour no- ticed in abdomen, but one was felt in the rectum	6 days, death	Ileum, cæcum, and colon into rectum.
114	E. Mayer, Percus- sion der Unterleibs, p. 85 (No. 140, Pilz)	8 years, F.	Blood passed; prolapse	11 days, death	Cæcum and colon into rectum.

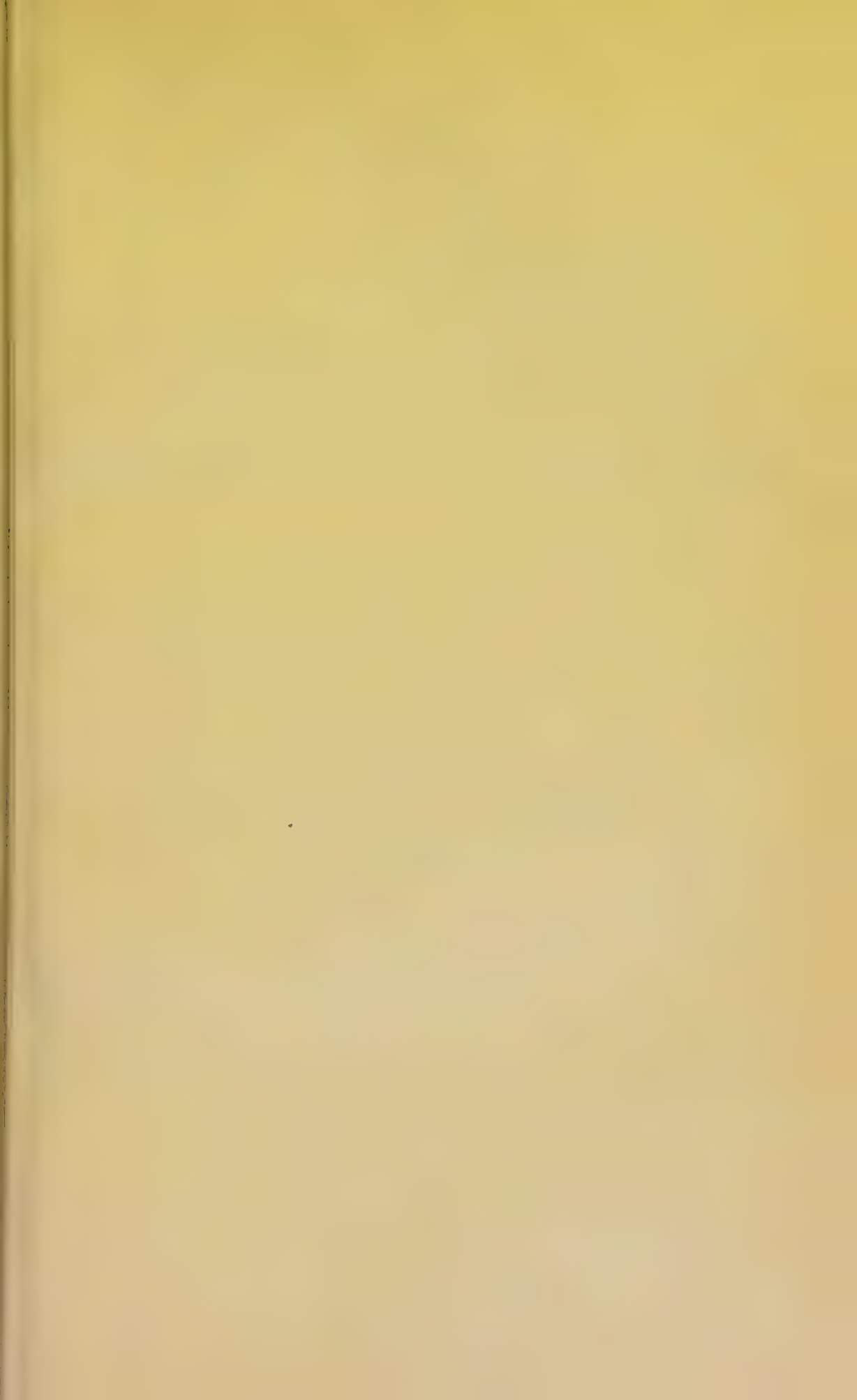
No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
115	Thomann, Hansen, No. 47 (148, Pilz)	11 years, M.	Prolapse	Some days, death	Ileum, cæcum, and colon into colon and rectum.	
116	Dr. H. C. Rose, Med. Times and Gaz., June 8, 1861, p. 597	5 mos., F.	Vomiting; passage of blood	3 days, death	Transverse into descending colon; there were no adhesions. The invagination was about four inches long.	
117	Abercrombie, l. c., p. 124	6 mos.	Vomiting; passage of bloody mucus; tumour in left side of abdomen; injections could not be made to pass up	3 days, death	Ileum and colon into sigmoid flexure; the included parts were very dark coloured, turgid, and in some places ulcerated.	
118	Bock, Schmidt's Jahrb., 146 Bd., 1870, p. 176	10 years, M.	Symptoms of intussusception, and on 17th day tumour detected on left side of abdomen; then it disappeared, owing, as was supposed, to subcutaneous injection of morphia. On 7th day after tumour reappeared; same treatment adopted. Finally symptoms of peritonitis. As the subcutaneous injections seemed so successful at first, no other treatment was adopted the second time	43 days, death	Ileum, cæcum, and colon invaginated into descending colon; perforation above intussusception had occurred.	
119	Spaeth, Virchow's Jahresb., 1869, Bd. ii, Abth. I, p. 138	36 years, M.	After two and a half months, blood stools, and tumour felt left of abdomen; then symptoms of peritonitis. It is said that no tumour could be felt in the rectum	6 mos., death	Ileum, cæcum, and colon into colon; perforation of the colon to an extent sufficient to allow the cæcum to protrude through it.	

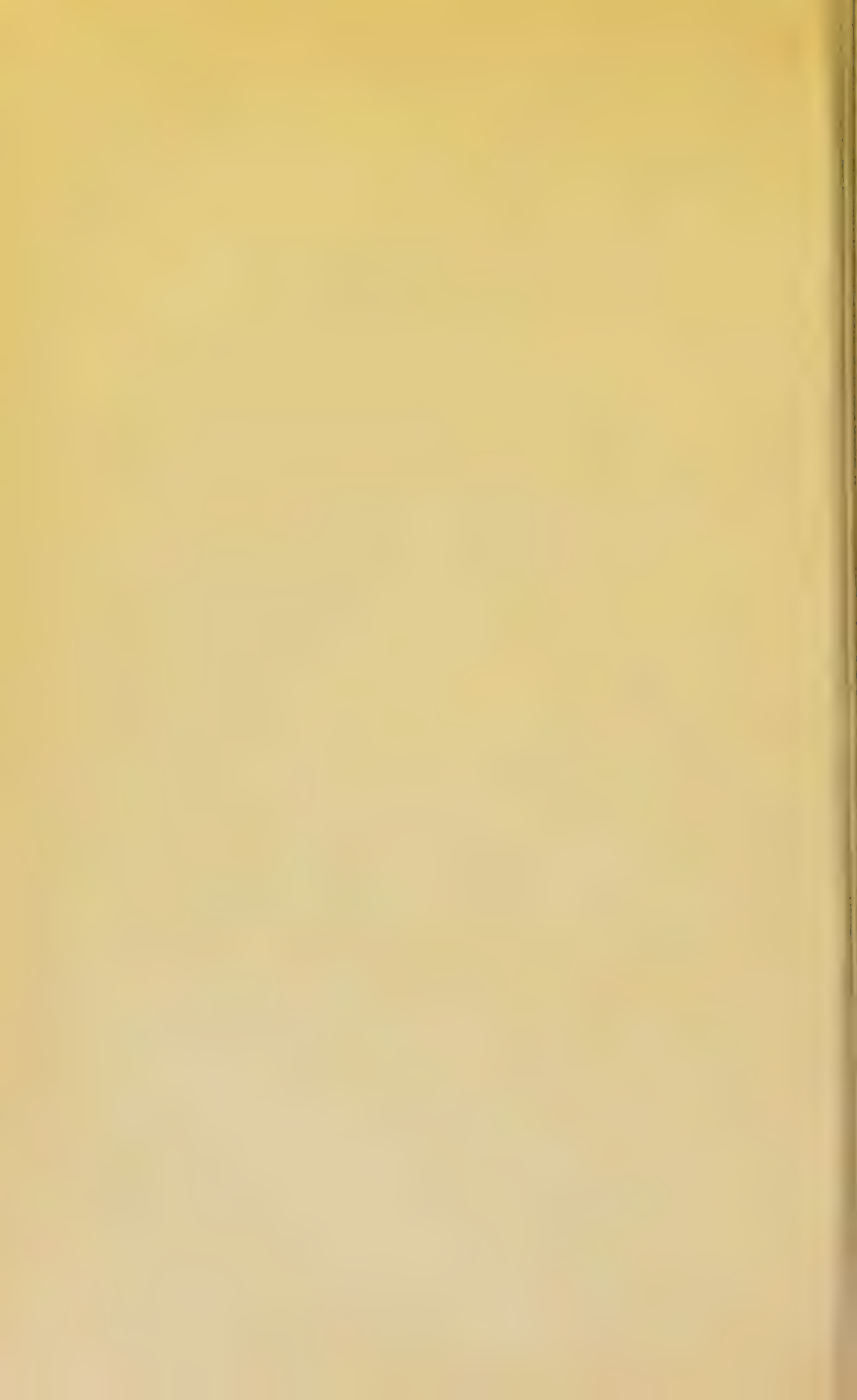
Med. Record, May, 1871, p. 115	domen, which changed position and became harder at times. The 2nd day, diagnosis made, and in- jection of air and kneading tried, but given up; then salt and water tried and continued. At first only four ounces could be thrown up; then, by evening, twelve ounces. On 3rd day six- teen ounces; and on 4th day, though bloody mucus passed, eighteen ounces, and the tumour disappeared. Morphia was given also	14 weeks, M.	Passage of bloody mucus; sick- ness; fulness on left side of abdo- men more distinct on the second day	3 days, death	that the ileum, artificially pro- truded through the ileo-caecal valve, could be pushed back by inflation. The attempt to force fluid through the valve, so as to reduce an invagination of the small intestine higher up, succeeded in two or three experiments on the cadaver with- out difficulty; in others it was impossible to send the fluid past the valve until a little manipu- lation removed the obstruction. He discussed and advocated the propriety of opening the perito- neal cavity in extreme cases, and applying taxis directly to the bowel affected
Dr. Plath, Cas- par's Wochenschr., 1839, p. 432		22 weeks, M.	Passage of blood; tumour felt left side of abdomen; sickness and constipation; tumour not felt per rectum; clysters were of no effect	4 days, death	Ileum, caecum, and colon into colon and sigmoid flexure, close to rectum. On attempting to withdraw the small intestine it tore at one part; there was no peritonitis.

No.	Reference.	Age and Sex.	Symptoms and Treatment.	Duration and Result.	Details of Autopsy or of Recovery.	Remarks.
123	Thomas, Journ. f. Kinderkrank., 1866, Bd. xvi, p. 23	6 mos., M.	Passage of blood; sickness; on third day no tumour; on fourth day tumour felt by rectum; a sound pushed the tumour back	3 days, death	Ileum, cæcum, and colon into colon and rectum; no peritonitis. Considerable force was required to withdraw the intestine, aided by a push from below.	
124	Ditto	23 weeks, M.	Passage of blood; tumour in abdomen second day: nothing felt by rectum; clysters, insufflation, and use of sound without avail	3 days, death	Colon ascending and transverse into descending; suspected to have been partially reduced; no peritonitis; no statement as to possibility of reduction.	
125	Ditto	16 mos., M.	Tumour left side of abdomen and prolapse; the latter was pushed back; the anus remained patent. The next day patient seemed better, but still passed bloody slime, and the anus was still patent. On the following day the anus closed; the peristaltic action was not re-established, probably owing to the long constriction	5 days, death	At the post-mortem the intestine was found to have been replaced	Dr. Schütz (Prag. Vierteljahrs., 1868, Bd. ii, p. 10) insists on the value of relaxation of the anal sphincter as a sign of intussusception.
126	Judson, Southern Med. and Surg. Journal (Gaz. Med., 1837)	5 mos., M.	Sickness; passage of blood; no note as to tumour in abdomen or in rectum. Death appears to have occurred within twelve hours of the passage of the blood and about twenty-four of the first symptoms	1 day, death	Ileum, cæcum, and colon invaginated into rectum, reaching six inches below the sigmoid flexure. It is noted that there was no peritonitis, nor adhesions, nor any effusion, but that the intestine was gangrenous (no description, only black from congestion?). An attempt to withdraw the invaginated bowel was	

127	Herbst, Rust. Magazin, Bd. xvi, p. 105 (44 Pilz)	20 weeks	Passage of bloody mucus ; tumour in abdomen	24 hours, death	Descending colon invaginated into the rectum.	unsuccessful, and the reporter remarks that if an operation had been undertaken the intestine could not have been liberated.
128	Forke, Untersuch. u. Beobach. über d. Ileus, &c., p. 39 (48 Pilz)	25 weeks, F.	Passage of bloody mucus	36 hours, death	Cæcum invaginated into the rectum.	
129	Wiegand, Hufe- land's Journ., Aug., 1830, p. 63 (50 Pilz)	6 mos., M.	Passage of bloody slime	2 days, death	Ileum and cæcum into de- scending colon and sigmoid flex- ure	Could have been felt?
130	Krukenberg, Jahr. d. ambul. Klinik., Bd. ii, p. 38 (97 Pilz)	More than a year	Passage of bloody slime ; tumour noticed in abdomen	18 days, death	Ileum, cæcum, and colon into the rectum.	
131	Roth, Wartzb. Med. Zeit., Heft 3, 2, 1862	28 years, M.	Sudden, severe pain over sym- physis ; shivering and fever ; then brownish, jelly-like motions. On the fifteenth day vomiting and prolapse of the rectum occurred, but was reduced. Afterwards a round, hard, ill-defined tumour was felt in the left iliac fossa, and on examination by the rectum an obstruction was met with pro- duced by an oblong, hardish tumour with a slit-like opening (resembling the os tinæ) at its lower end	3 weeks, recovery	The treatment consisted of the use of long-continued warm baths, frequent injections of luke- warm water, and the administra- tion of castor-oil occasionally during a portion of the time. Daily examinations were made per rectum, but it did not seem that the injections had any effect ; finally, the tumour disappeared. It is noted as a case of sponta- neous reposition ; the attempts to effect reduction by surgical pro- cedures being unavailing	Supposed spon- taneous reposition.







4

NOTES OF A SECOND CASE

OF

ABDOMINAL SECTION

FOR

INTUSSUSCEPTION INTO THE COLON,

WITH REMARKS ON THE DETAILS OF THE
OPERATION.

BY

JONATHAN HUTCHINSON, F.R.C.S.,

SENIOR SURGEON TO THE LONDON HOSPITAL; SURGEON TO THE ROYAL LONDON
OPHTHALMIC HOSPITAL, MOORFIELDS; AND TO THE HOSPITAL
FOR DISEASES OF THE SKIN.

Read December 14th, 1875.

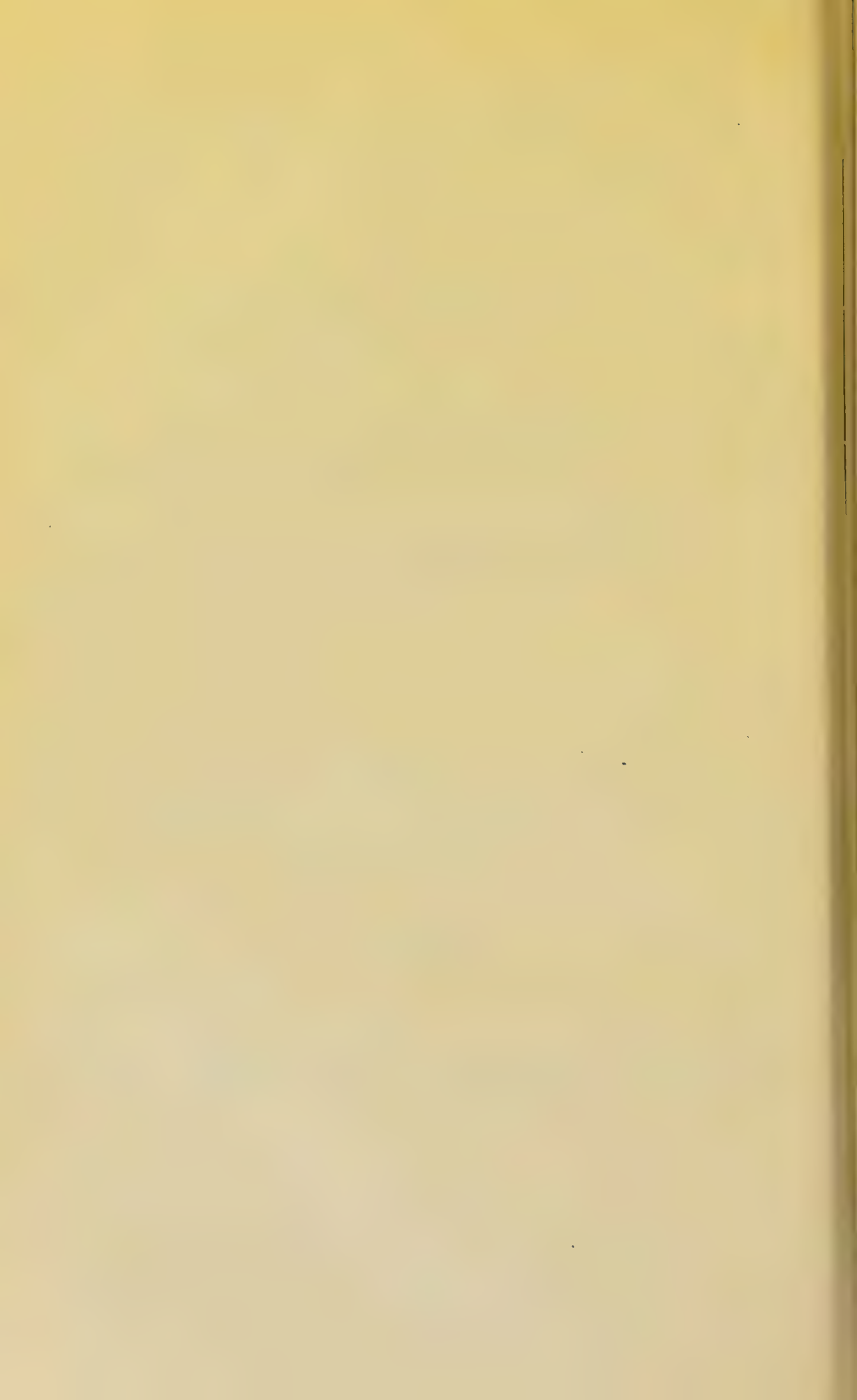
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1876.



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Received December 1st—Read December 14th, 1875.

A SECOND case of operation for intussusception, which occurred to me a few weeks ago, gave me the opportunity of acquiring additional experience as regards some points of detail which it is, I think, of importance to bring before the profession.

The patient was an infant, aged 6 months, under the care of Dr. Madge, by whom I was asked to see the case on the fourth day of the illness. The symptoms had from the first been well-marked, the child having experienced frequent attacks of painful straining with the passage of bloody mucus. The invaginated part could be easily felt in the left flank by manipulation through the

abdominal wall, and also by the finger introduced into the rectum. It had not been actually extruded, but its lower end was often within an inch of the anus. Dr. Madge had fully diagnosed the case before I saw it, and the usual measures of treatment by injection, &c., had been carefully employed. An infant sister of the child had died of the same lesion about a year previously. When I saw the infant it was evident that there was no time to be lost, for it had rejected all food for three days. It was very weak and did not seem likely to live many hours; we therefore persuaded the parents to allow us to perform the operation at once. I found the operation, as perhaps might be expected at so early an age, much more difficult of accomplishment than in my former case, and it is chiefly to these difficulties that I wish to ask the attention of the Society. The abdominal wall was thick, being loaded with fat, and the space between the umbilicus and pubes was very short. Having made an incision in the *linea alba*, just sufficient to admit two fingers, I was able easily to reach the neck of the intussusception, but I found it so slightly moveable that I could not by any means hook it into the wound. I was very anxious not to allow the intestines to escape through the wound, and finding that I could draw the bowel out to a certain extent between the tips of my two fingers, I spent some time in trying to accomplish reduction in this manner, hoping to complete it without exposing the bowel to view. At length, however, I became convinced that this was impracticable; only a certain length could be drawn out and then it became firmly fixed. I was obliged, therefore, to enlarge the wound freely above the umbilicus, and to allow the intestines, much distended with gas, to escape. It was only when the abdomen was almost empty that I could bring the neck of the sac into view in the wound, and I then made repeated attempts to draw the bowel out, but without success. That there were no adhesions was proved by the fact that an inch or two could be easily drawn out; the impediment was clearly due to the

ensheathing bowel being thrown into folds by traction, and thus constituting a series of strictures which gripped its contents. In this dilemma, and when almost in despair as to whether I should accomplish the reduction, it occurred to me to seek the lower end of the invaginated part, and try to hold the ensheathing layer so as to prevent its being drawn into folds. This led to the discovery that it was only the neck of the invaginated tract that was in any degree fixed (by its mesentery); the lower part, consisting of the sigmoid flexure of the colon much elongated, lay in loose folds on the rim of the pelvis, and was hooked out of the wound with the greatest ease. The attempt to hold the ensheathing layer straight at once revealed the true method of reduction, for by pulling this downwards, instead of trying to pull the involved part upwards, I accomplished the replacement with the greatest ease. The *appendix vermiformis* came out last, just as the reduction was completed, proving that the intussusception had begun at the cæcum. The operation had been protracted and the infant was almost pulseless, but there still remained the task of replacing the intestines within the abdominal cavity. I had some difficulty in accomplishing this, and several times after their return, portions escaped again before I could close the abdominal wound. Under these circumstances I was induced to prick the distended small intestine at two or three places with a harelip-pin to allow the escape of flatus. To my chagrin, however, just as I was closing the abdominal wound I observed that one of the punctures in the bowel was bubbling, and that there was a stain of feculent matter on the peritoneal surface; this was carefully wiped away and the wound closed. The child, although in extreme collapse when the operation concluded, rallied subsequently, became warm, took the breast, and passed a feculent motion. Death, however, took place in the night, about eight hours after the operation.

At the *post-mortem* next day we found no trace of any

further escape of fæces, but there was almost universal peritonitis, the coils of intestine being glued together by lymph. It became of interest, as one of the child's sisters had died with precisely the same condition, to ascertain whether there was any congenital peculiarity which might account for its occurrence. We found the parietal peritoneum in the right flank quite smooth and entire, whilst the cæcum hung very loosely attached by a long mesentery. This had probably been much stretched in the process of invagination, but there could I think be no doubt that the cæcum had been loose congenitally. No reproduction of the invagination had occurred.

It is my impression, judging from my experience of this case, that in future operations the lower end of the invaginated tract ought always to be first sought, and that reduction ought to be accomplished by squeezing it or pulling the sheath downwards rather than by attempting to pull the contained tube out. I am not at all sure that in some cases this might not be accomplished without bringing the parts into view; should this not be found practicable, however, it is probable that the operator will find it much more easy, in cases of intussusception into the descending colon, to bring the lower part into the wound than the upper one. It is the more necessary to draw attention to these points because they are both probably unlikely to occur of themselves to the operator's mind.



**On the Transmission of Syphilis from Parent to Offspring,
with an Appendix of Cases. By JONATHAN HUTCHINSON,
F.R.C.S., Senior Surgeon to the London Hospital.**

THAT acquired syphilis developes itself with exceedingly different degrees of severity in different persons all will admit. For the most part we are obliged to accept this fact without explanation, and to say simply that in itself it proves that individuals differ much in their degree, or kind, of susceptibility to the syphilitic virus; or perhaps we ought to express our meaning in somewhat different words, and say that the tissues of different persons show differing tendencies under the influence of syphilis. For there are a class of facts which show that it is not so much differing susceptibility as different kinds of reaction. Of twenty persons who may show the disease in exceedingly different phases of severity it may be true that no one differed from the others as regards susceptibility. All contracted the malady with equal ease, all went through the same stages, and all were equally protected by it as regards future attacks; but how different may have been the kind of manifestation. One may have had a sore which lasted, it is true, a month or two, but which gave him no trouble, and which was not followed by anything which could be recognised as a constitutional phenomenon, whilst another may be covered from head to foot with eruption, have lost his hair, had sores in the throat, iritis, and a state of general ill health which laid him up for months. That the one as truly went through the stages of the syphilitic fever as the other is proved by the almost equal liability to tertiary symptoms, and the equal risk in each of transmission to offspring. Some physicians—Dr. Wilks, I think, for one—to whom we are much indebted for investigations as to severe visceral forms of tertiary syphilis, holds that it is exceptional in these cases to be able to obtain any good history of secondary symptoms. Without adopting this opinion, and whilst believing that the secondary and tertiary symptoms are usually as regards severity in direct rather than in inverse ratio, it is yet of importance for my present purpose

to allude to it, and to admit that it is one for which much might be said.

It is not true that this varying severity in the secondary stage is to be explained by reference to the patient's state of health, temperament, age, sex, or diathesis. The most robust are often those to suffer most severely ; and though we may admit that syphilis does acquire certain minor differences in connection with the age, sex, and temperament of its victims, yet these certainly go but very little way in explanation of the startling variations in severity to which I have adverted. That the secondary symptoms are usually in ratio with the primary seems probable, at any rate it is very rare for a patient who has had a chancre of unusual development in respect to size and hardness to escape, excepting as a consequence of treatment, a severe outbreak of eruption, &c. In saying this, however, we say little more than that the manner and degree in which a chancre indurates may be taken as a specimen of the manner in which the cell-structures of the individual are likely to behave when brought under the influence of the syphilitic virus. Apart from this we possess no knowledge which will enable us to predict during the first stage of syphilis or before it as to any given person whether he is likely to suffer severely from syphilis or not. It would be as easy for a physician to pick out those who will have smallpox badly. Both in the case of the medical exanthems and of syphilis it would appear that the difference in constitution which determines severity or mildness may be a very slight one, for it is common enough to witness that, of brothers or other near relatives apparently of similar temperaments and in like conditions of health, one may suffer very severely and another escape with a very slight illness.

I feel sure that it is almost impossible to estimate too highly the importance of this fact, that acquired syphilis is, for reasons which we cannot explain, very unequal in its incidence in different persons. From want of giving it due weight we are exposed to endless fallacies in our inferences, both as to the natural history of this disease and the results of treatment.

A precisely similar line of argument is applicable to inherited syphilis. We do not yet know within what limits of severity or of mildness the disease may vary. Our investigations here are even more beset with difficulty and uncertainty than in the case of the acquired disease. We do not, in the first place, know whether inherited taint is always the same kind. It has been customary to assume, as regards inheritance, that the child born nearest to the parent's taint is the one most likely to suffer severely, and the younger, if they do not escape entirely, may perhaps suffer only slightly. But a possible fallacy under-

lies our creed in this matter which it is very desirable to expose. It may be that, after all, there are no degrees of severity in inheritance, no varieties in the potency of the virus received by the child, and that if a child inherits any taint whatever it inherits, as a necessity, the whole disease. It is possible that the extremely unequal severity with which different children suffer is to be explained rather by differences in idiosyncrasy than by supposing that one received a large dose of the poison and another a small one. That this is really so may be made probable if we can show that of children born within equally short periods of the parental contamination the range of variation in severity is as great as in those more remote. It is with the hope of supplying some evidence on this and on many other points in respect to the facts as to inheritance of taint that I have collected the following cases from my note-books. They are purposely put as concisely as is possible consistently with clearness of statement. Isolated cases prove but little, and, being firmly convinced that it is only by the most liberal citation of evidence that we can hope to arrive at truth, I make no apology for trespassing on the reader's patience with so many case narratives.

I cannot better illustrate the apparent irregularity with which the taint of syphilis is transmitted to children than by mentioning two cases which were brought recently under my notice on the same day. On the morning in question a surgeon from the country consulted me respecting his own case. He had married three or four years after syphilis, having been under treatment in the first instance for about six months, and having believed himself quite well for two years or more. His wife never suffered, but her first two conceptions ended in dead births, and the next two children, although born alive, died with the usual symptoms of inherited disease. Seven children now living have all in turn shown symptoms of infantile syphilis. Thus, it would appear that eleven conceptions have in succession been tainted, the inheritance being from the father only, he being in fair health, and a period which has now reached fifteen years having elapsed since he contracted the disease. Let me mention my next case in contrast. On the same morning my friend Dr. Fletcher, of Camden Town, brought to me a married lady from the country whose antecedents he was well acquainted with. This lady's husband had contracted syphilis during her first pregnancy, and been treated for it by Dr. Fletcher. His wife about six months later showed the usual symptoms—had rash, lost her hair, and had sores on the tongue. Her primary symptoms had been neglected, her husband not choosing to run the risk of exciting her suspicions. After this

Mrs. — has remained under Dr. Fletcher's observation at times until the present date, and she still shows numerous sores on the sides of the tongue and at the angles of the mouth, and has a few patches remaining on the skin. It was for these persistent symptoms that she was brought to me. Now, the remarkable fact as to inheritance is this, that this lady has, during the last two years, borne two children, and that neither of them has shown any symptoms. It will be seen that they are the offspring of parents both of whom have suffered within three years, and one of whom (the mother) still shows symptoms indicating considerable activity in the virus. I have been careful to say merely that they have shown no symptoms, not that they have escaped, for I do not believe that they have escaped. I should almost regard it as impossible that children could be born under such circumstances and escape a taint. Without, however, believing that they have really escaped, the two series of facts may serve well enough to illustrate my point as to very unequal severity in the incidence of the disease, and to suggest a source of fallacy in our observations which we ought constantly to keep in mind, and for which it is almost impossible to make too much allowance.

I. *Inheritance from Father and Mother*.—Case of Esther R—, 46, and daughter; (Bag "Syph. Paralysis").—Mother had an eruption six weeks after marriage, which her husband told her was caused by venereal disease from him. This was sixteen years ago; two years ago she had sudden *left* hemiplegia, and paralysis of *right* fifth nerve; was unconscious for three weeks. Gradually recovered, but had a relapse on same (left) side, and again has nearly recovered power of left arm and leg, and partially sensation of right face; right eye lost. No details as to husband.

1. F., æt. 14, no history of infantile symptoms, but a doubtful account of keratitis.

2. F., died at birth.

3. F., æt. 9, now under my care. Periostitis of left tibia; double keratitis; typically notched teeth.

4. F., died at birth.

5. F., died at birth.

There were also two miscarriages at about three months, the dates of which in reference to the other pregnancies is not noted.

II. *Inheritance from both parents*.—Anne H—, 4, M. F. "O," p. 128, and Honora H—, M. F. "O," p. 140.—Mother had primary and secondary syphilis eleven years after the birth of her first child. No note of father.

1. Reported to be quite healthy. Now mother's syphilis. Since then seven other children. Of these five have died: one

at three weeks; one at one week; two born dead; one at two weeks; two are living, *viz.* Honora (see reference above), æt. seven years. Typical aspect and nose. Sores in mouth from sharp teeth. Anne, æt. four years. Scars about anus and on legs ("? small-pox") and strumous ophthalmia.

III. *Inheritance from both parents.*—Sarah G—, 26, and son, M. F. N., Nos. 2896, p. 133, and 30, 58.—Mother married eight years. Good health till then, but failed afterwards. No history of primary or definite secondary disease. Had sore tongue and mouth; then four years after marriage serpiginous eruption on knees; then nodes on tibia, and now enlargement of right femur, rapidly diminishing under iodide. No note of father.

1. Æt. eleven, reported to be healthy and never to have ailed anything.

2. Died, æt. five weeks, with snuffles and rash.

3. M., James G—, æt. 6. Had severe snuffles and rash, and a condyloma on tongue, which was burnt, and scar remains. Now kerato-iritis.

The evidence as to the father is negative; the wife may have acquired it elsewhere after marriage, but her history is more that of disease from conception.

IV. *Inheritance from Father?*—Case of Flora C—, B. 33, and my book, p. 168.—No history of syphilis in either parent to be obtained from mother. She thinks her husband may have had it before marriage.

1. F., died at one month.

2. M., died at birth.

3. F., died at birth.

4. F., died at nineteen months.

5. F., died at four months.

6. Died at ten months, "atrophy."

Several of these had blotches, &c.

7. Flora C—, 10. No history of infantile symptoms. Now most typical teeth and physiognomy. Lately, extensive choroido-neuro-retinitis in each eye, and partial idiocy.

8. F., died at ten months.

9. F., seven years old, "healthy."

Remarks.—Persistency for seven years, at least, very probable.

V. *Inheritance from Father and Mother.*—(H— family. M. F., vol. 1, case 207; vol. 3, pp. 63 and 139; vol. O, p. 106; vol. M, pp. 165 and 292 and 107; vol. K, p. 29.)—Father had primary syphilis and suppurated buboes at æt. 20, two years before marriage. Now (æt. 27), has psoriasis palmaris. Wife had no symptoms till after birth of first child, when she suffered from sores and condylomata.

1. M., Edward H., æt. 5, Sept. 29, 1857. Had snuffles for three or four months, and was puny till æt. two years. Then "large patches of psoriasis" on face and chin, which afterwards became "slightly tubercular," and was cured by iodide. Then at æt. 5 condylomata at anus.

2. M., John W. H., at æt. 5 weeks; "beginning to waste," and commencing "syphilitic psoriasis" January, 1856.

3. M., Thomas H., æt. 6 months (Oct. 1859). Snuffles, severe cachexia, psoriasis, &c. Syphilitic onychia (*nails sketched*).

Remarks.—It is possible from the above statements that the mother contracted primary syphilis after she had borne a syphilitic child. If so we should have an exception to Colles' law. But against such a supposition is the fact that if it were true we should have to suppose that the husband had fresh syphilis within a short period of his first attack. I have thought it best to let the notes stand as I made them twenty years ago, but my present belief is that the wife must have been mistaken in her history, and that probably what she described as her first symptoms were only a relapse.

We have proof of persistence of transmission for seven years, and I believe that it extended much longer. Mrs. H., who suffered most severely, was under my observation for ten years or more, and brought me child after child all suffering with equally definite and severe symptoms. The last that I saw suffered just as severely as the first. All were born alive, and I believe that under treatment all survived. I regret that my notes do not supply details of more than the above three.

VI. *Inheritance from Mother only.*—Bag "Hered. Syph.," drawer 34. Joseph A—, 3.—Mother caught syphilis from a nurse child seven years ago and between her third and fourth pregnancies. She had a sore on nipple and copious secondary symptoms; good health since. No note as to father.

1. M., died at ten years of "phthisis." No history of syphilitic symptoms.

2. Living, 8½ years old; reported good health.

3. Died of "fever."

Now mother's syphilis.

4. Died at three months, "gastric fever." History of syphilitic symptoms.

5. Joseph Andrews, 3. Had bad snuffles and rash and swellings on head, which did not break. At three months, inflamed eyes resulting in occluded pupil in the right (iritis). Scars at angles of mouth, head large, nose flat, upper incisors decaying.

6. Died at seven months.

Remarks.—Death-rate two in three before the syphilis, and the same subsequently.

This case and the following are alike, in that the mother contracted syphilis accidentally and at a known date. There is every reason to believe that in each the inheritance was from her only. In Mrs. D.'s case I myself saw and treated her chancre. Both women had borne children before the accident. It is a warning lesson against hasty inferences to note that in each the infantile death-rate had been very heavy *before* the disease was introduced.

VII. *Inheritance from Mother only.*—Mrs. D—, 36, and infant M. F. L., pp. 13 and 384, and K., p. 32.—Mother contracted syphilis by nursing a neighbour's child, which shortly afterwards died of the disease.

She had a characteristic hard chancre on the nipple, followed by rash and condyloma. She had weaned her own baby just about the time when she began to suckle the neighbour's child. Her own baby remained healthy. The neighbour's child had a badly ulcerated mouth.

Born before mother's syphilis :

1, 2, 3, 4, 5, 6, 7, four dead (no details); three living, and reported healthy.

8. Living and healthy, weaned just before mother caught syphilis.

Now mother's syphilis; chancre, December, 1857; rash, &c., to June, 1858.

9. Born dead in October, 1858.

10. M., George D., æt. 1 month, born September, 1859, at eighth month. Was very ill at birth, and soon wasted; under care for severe syphilis at æt. one month, and died soon after.

VIII. *Father.*—George P—, 47, and son, G. 313.—Father denies ever having had any venereal disease, but admits risk before marriage. He is under care for cycloplegia of one eye (incomplete) and had old choroiditis in the other.

1, 2, 3, 4, 5, and 6. All F., all alive, and reported to be and to have been healthy. None are deaf, and none have had bad eyes.

7. M., died at æt. three years, of "smallpox"; all the others also had the disease, but the parents did not take it.

8 and 9. Miscarriages.

10. M., æt. 9; now under care for severe double keratitis, with a small hypopyon in one eye. Teeth suspicious.

Remarks.—It may be plausibly suspected that the disease was introduced between the seventh and tenth birth.

IX. *Father and Mother.*—Mary B—, 14, M. F. J., p. 1631.—There is here no history of syphilis in father, and only an account of some sores about genitals and anus in mother "after one of her early confinements." The husband, a few years after marriage, was found to be living with another woman, and from the facts

as to the children, it becomes nearly certain that syphilis was introduced by him between his wife's first and second pregnancies.

1. F., æt. 18, well grown, florid, and shows no signs whatever of syphilis.

2. Born dead.

3. Died at nine months, "water on the brain."

4. F., Mary B., æt. 14, extremely marked syphilitic physiognomy, typical teeth, nearly blind from most severe keratitis and iritis, for which she attended hospitals between æt. 6 and 10. Not deaf. History of nodes on head. Very severe rash and illness in infancy.

5. Died at two weeks.

6. Died at ten months.

7. Living, æt. 11 years. Forehead large, but nothing characteristic of syphilis in physiognomy. Is delicate.

8. F., died at eight months.

9. F., died at ten months, with "the head."

10. M., Living, æt. 6 years. Reported healthy.

11. F., died at ten months.

The facts as to the cause of death of several of the infants are wanting, but there is no proof that the disease persisted long in the family. It is not certain that more than one suffered, although the two following deaths may be suspected.

X. *Inheritance from Father* (G. 211).—Father had syphilis at 17, and has since had much psoriasis, benefited by mercury; now has nerve symptoms. (The above is from the surgeon who has attended him throughout.) Mother is the subject of well-marked hereditary syphilis, with keratitis; is eighth of thirteen, nine of which were stillborn. One of the four living ones has fits and is partly out of his mind. She has had no disease from her husband (direct questions).

1. M., æt. 12, now has interstitial keratitis of one eye and choroiditis in other. History of severe infantile symptoms.

2, 3. Born dead.

4. Living; said to have had no symptoms.

5. Born dead.

6, 7, 8, 9. Living, and reported to have had no symptoms.

10. Born dead.

Of the six living five are males, one female.

Of the four still-born two were males, two females.

Remarks.—In this and the following case it of some interest to note that the mother was herself the subject of inherited taint. Had it happened that the history of disease in the father had not been forthcoming it might have been assumed that the child inherited from her mother, and that we had proof of transmission to third generation. All facts seeming to support such a supposi-

tion must be received with much doubt, unless the history of the other parent is very clearly negative.

XI. *Inheritance from Father*.—Case of Sarah F—, F. 374.—Date of father's syphilis not ascertained; has been "very gay," and is now (æt. 53) hemiplegic. Mother married to him thirty-one years, and now 53; has several times had gonorrhœa from husband, but no history of syphilitic symptoms. *She is herself the subject of hereditary syphilis.*

1. Miscarriage at third month, twelve months after marriage. Now the interval of twelve years without a pregnancy; during this time and three years after marriage husband gave her gonorrhœa for first time.

2. M., born dead at full time.

3, 4, 5, 6, 7. All miscarriages.

Remarks.—Not a single viable child, but no proof that any were syphilitic.

XII. *Father, probably*.—Case of Mary Anne B—, 9, G. 319.—Father reported by mother to have had good health till after birth of first child. Between first and second child he became ill of "rheumatism." Was very ill for a year and a half, and never well afterwards, but no history of syphilitic symptoms. He died at 43 of "bronchitis" and "consumption." Mother robust and healthy, appears to have had no symptoms.

1. M., living, 23, always had good health. Now father's illness, probably syphilis, with severe rheumatic symptoms.

2. M., died at fourteen months; a "healthy fat baby," died after six days' illness; "teeth and inflammation."

3. Born dead.

4. M., died at first month. "Delicate."

5. F., died at fourteen months. Weakly; "abscess on shoulder."

6. F., died at five years. "Diseased bone behind ear," probably result of "measles and scarlet fever."

7. M., died at eight months.

8. F., died at eight months.

9. F., Mary Anne B—, æt. 9. Interstitial keratitis of right. Extremely typical teeth. Slight deafness. Moderately characteristic physiognomy. Scar of an abscess under right lower jaw (probably strumous).

It is very probable, but not certain, that the father had syphilis. The great mortality is remarkable.

XIII. *Inheritance from Father* (F. 398).—Father had venereal disease, with bad throat, twelve years ago. Has had multiple round ulcer on right leg since primary disease, and now has paresis of left sixth nerve and noise in left ear of three weeks' duration.

Wife has had seven children, six M., some of which were born dead, and all the rest died within a few days of birth.

1. F., living, æt. 8. She shows no signs of syphilis, but has not yet cut permanent incisors. She is about the middle one of the seven.

Remarks.—But one child living out of seven. All died within a few hours of birth, but there is no absolute proof of syphilis in any.

XIV. *Inheritance from Father.*—Case of Mr. P—, 44. Priv. H. 197.—Father had severe “gonorrhœa” twenty years ago, lasting many weeks (twelve years before marriage). No history of secondary symptoms or of a chancre. He has, however, characteristic white marks on tongue and inside cheeks. Married eight years. Wife appears not to have suffered.

1 and 2. F., both living and healthy; had no symptoms.

3. M., died at a few months. The surgeon said it was syphilitic.

4. M., born prematurely; died at two weeks.

5. M., living and well.

6. M., died at six weeks. Was covered with “syphilitic rash.”

Remarks.—This case is difficult of interpretation. It is difficult to avoid the suspicion that the taint was introduced by one of the parents between the birth of the second and third, probably by the father. If this be so we have three deaths out of four.

XV. *Inheritance from both Father and Mother.*—Case of Edward H—, 9. G. 146.—Father “gay;” wife knows he “had venereal disease several times, both before and since marriage.” Details wanting. Wife has had vaginal discharge repeatedly, and some history of *recent* secondary symptoms.

1. M., a seven months’ child; died at birth.

2. M., Edward H—, 9; a seven months’ child. Had bad snuffles; no rash. Interstitial keratitis; typical teeth. Characteristic physiognomy. Badly grown. History of purulent ophthalmia in infancy.

3 and 4. M., Twins, born dead at six and a half months.

5. M., died at eighteen months, “teething.” Thrush and snuffles. A seven months’ child.

6. M., died at birth. Seven months’ child.

7. F., living, æt. 14 months. Had snuffles. Now pale and badly grown.

Remarks.—The taint has lasted through the whole family, unless in one or other parent it has been reintroduced.

XVI. *Inheritance from both Father and Mother.*—Mrs. J—, H. S. D. Daughter at Moorfields, for keratitis, Nov., 1873.—Husband had syphilis between the second and third child; treated and

got quite well, and has remained so ever since. Mother had it from him during her fourth pregnancy; she had it mildly, but five years later began to suffer from severe ulceration of nose, leading to loss of bone and destruction of the alæ. Cured by iodide at Skin Hospital, and has remained well for the last six and a half years. Was my patient at Skin Hospital.

1. F., 16. Good health.

2. Miscarriage.

3. M., 14. Good health.

During next pregnancy the syphilis occurred. It is not stated at what period of pregnancy the mother became infected.

4. F., 12. Had snuffles, and gave her mother much trouble in infancy. At æt. 12 had typical interstitial keratitis; under my care at Moorfields. Typical syphilitic teeth.

5. M., 11.

6. F., died at ten weeks.

7. M., 8. In good health.

8. F., 6. In good health.

9. F., 5. In good health.

10. M., 4. In good health.

11. F., 9 months. In good health.

Several of these had snuffles in infancy, but none ailed much.

Remarks.—It may be doubted whether the younger children have really inherited any taint. The single symptom of snuffles estimated only by the mother cannot count for much, still it is possible that it was of syphilitic origin. It is an instance of a child *in utero* contracting syphilis from its mother, but unfortunately the stage of pregnancy at which the disease was acquired is not known.

XVII. *Inheritance from both Father and Mother.*—Case of Lillian W—, 5. F. 388.—Mother had venereal disease, with spots on thighs, a month or two after marriage. No positive history of symptoms in father.

1. F., born dead.

2. F., Lillian W—, 5. Had snuffles in infancy; now has interstitial keratitis and suspicious physiognomy.

Remarks.—We have here the first born, after recent disease in both parents, killed probably by it, and the second suffering severely, but surviving.

XVIII. *Inheritance from both Father and Mother (?)*.—Case of Eliza T—, 2. G. 16.—Mother had sores on genitals and spots on thighs three weeks after marriage; no history of general rash, but her hair came out. No particulars as to date of syphilis in father.

1. F., born dead at seventh month.

2. F., born dead at eighth month.

3. F., Eliza Taylor, 2. Had snuffles, thrush, and eruption on head and buttocks. Now has choroido-retinitis in each eye.

4. M., æt. 6 weeks. Reported healthy.

P.S.—Two and a half years after above notes.

5 and 6. Born since above notes, and reported healthy.

Remarks.—As in the preceding case, we have first dead births, then a child suffering severely but surviving, and then healthy children.

XIX. *Inheritance from Father and Mother* (F. 347).—Case of Mrs. M—, æt. 36.—There is no statement as to syphilis in the father, but there can be very little doubt. The mother, who has been married to him fifteen years, and seems never to have had symptoms of syphilis till nine months after the twelfth confinement, when she came under care for paralysis of one third nerve, which passed off in two months under iodide.

1, 2. M., born dead.

3. F., born dead.

4, 5, 6, 7, 8, 9, 10, 11. M., all born dead.

12. M., æt. 10 months. Had bad thrush, eruption and snuffles, and now has a somewhat prominent forehead, depressed nasal bridge, and sore at angle of mouth. No condyloma.

Remarks.—A good instance of what is supposed to be very common—a long series of dead births due to taint. If we may assume that syphilis was the cause, and that no second introduction of the disease occurred, we have proof that transmission may occur twelve or fifteen years after the primary malady.

XX. *Inheritance from both Father and Mother.*—Amelia L—, æt. 9 (M. F. N., p. 174).—Father has now badly cracked syphilitic tongue. Mother no symptoms, except pains in bones.

Four pregnancies; one miscarriage (date not given), three born alive, as under:

1. F., Amelia L—, æt. 9. Treated by Mr. Startin in infancy for rash on buttocks, &c.; had no snuffles; now kerato-iritis (began with unusually well-marked iritis, the corneal haze supervening shortly after).

2. Living, and reported healthy.

3. Died at two months. Was a six months' child.

XXI. *Inheritance from both Father and Mother* (S. H. Letters, 1868-70, 246).—Harriet T—, æt. 44.—During the last four years mother has had several large gummous tumours on left arm and elsewhere. In other respects has had good health since marriage. Husband reported healthy, but no details as to whether the syphilis was derived from him.

1. M., died at four months.

2. F., æt. 14; has notched and screw-driver teeth, but nothing else.

3. F., æt. 13. No signs of syphilis in teeth or physiognomy.

4. M., died at four months.

5. M., died at two months.

6, 7, 8, 9. All F. Living, and reported healthy; one who comes shows no signs.

Remarks.—The children were brought up by hand, away from home, and mother knows nothing of the symptoms in the third (M.), who died in infancy. In this instance the taint appears to have ceased, and the younger children have shown no symptoms.

XXII. *Inheritance from Father only.*—S. H. Letters, 1875, 375. James L—, 43.—Father had a chancre, for which he took pills till his mouth was sore, eight years ago. Had no secondary symptoms. Comes for pityriasis palmaris of one hand, which has rapidly healed under bichloride of mercury.

The primary disease occurred after death of his first wife and one year before he married a second time. The conceptions terminated as follows:

1, 2. Miscarriages at third month.

3. F., died at birth.

4. F., died at two weeks.

5. F., living, æt. 9 months; reported to have snuffles.

Remarks.—Here we have two miscarriages, a dead birth, and an infant which died early, and then a living infant which showed symptoms. An instance of too early marriage after syphilis.

XXIII. *Both Parents.*—L. H. Letters, 1868-70, 468. Emma P—, 32.—Mother suffering from serpiginous tubercular eruption on forehead, of nearly two years' duration. It began about five months after first confinement, probably by conception. No note as to husband; married three years.

1. F., born nine months after marriage; died at two days.

2. F., living, æt. 1½ year. Had snuffles, but no thrush. Bridge of nose depressed, frontal eminences prominent, upper incisors have already dropped out.

XXIV. *Father.*—F., p. 154. Case of Maria D—. Father now in late tertiary stage; has lost bone from hard palate and had extensive ulceration of soft palate and uvula; dates of syphilis and marriage not given. No note as to mother.

1. born dead.

2. M., living and reported healthy.

3. Born dead.

4. M., living and reported healthy.

5. F., comes to hospital at request. No signs of syphilis; no history of past symptoms.

6. M., living and reported healthy.

7. F., Maria Duff, 13. Typical teeth and symmetrical interstitial keratitis and iritis; deafness.

8, 9. F.

10, 11, 12. Miscarriages.

13. M.

14. F.

15. M., æt. 6 weeks. Reported to have thrush.

Remarks.—In all probability the disease was acquired by one or both parents between the sixth and seventh births. The subsequent notes are imperfect.

XXV. *Inheritance from Father probably.*—Case of Matilda J—, 19, F. 304.—No conclusive evidence as to source of the syphilis. Mother married twice, and several of the children by the second husband furnish more or less conclusive evidence of the introduction of the disease by one or other parent at that date.

By first husband :

1. F., æt. 27. Shows no signs of syphilis.

By second husband (probable date of introduction of syphilis) :

2. M., died in infancy.

3. F., died in infancy.

4. F., Matilda J—, 19. Interstitial keratitis; screw-driver teeth; scarred angles of mouth; chronic synovial thickening of left knee-joint.

5. F., living, æt. 17.

6. F., living, æt. 15.

Said to be, and always to have been, in good health.

Remarks.—The taint would appear to have ceased with the fourth child.

XXVI. *Inheritance from Father.*—G. 301.—Father had well-marked syphilis about eighteen years ago (date in relation to marriage not stated). Was salivated for the secondary symptoms. Now paresis of right third nerve.

1, 2, 3, 4, 5, 6, 7. Miscarriages.

8. F., æt. 13. Is very stunted; looks only eight. Physiognomy suspicious, but not more. Teeth normal. *A single abruptly defined patch of choroidal atrophy* at periphery of one eye.

9. M., æt. 8. Healthy looking and well-grown.

10. Living, reported healthy.

The choroidal patch is important and interesting evidence.

Remarks.—It is probable that the child was born within five years of her father's syphilis. No facts as regards the escape of the mother are recorded. Four years after the birth of the tainted but viable child we find healthy infants,

XXVII. *Father* (?).—Case of Mary A. S—, syphilitic keratitis. —Father a sailor. Mother knows of no suspicious symptoms either in him or herself. He is troubled with “abscesses.” Probably the father had the disease before the birth of the fourth child.

1. M., 28.

2. F., 26.

3. F., 24.

Conjectural date of syphilis.

4. Born dead at eight months.

5. Born dead at seven months.

6. F., 20, Mary Anne. Has still the remains of keratitis, which occurred at *æt.* 9. Teeth typical.

7. F., died at six weeks “from a small bleeding sore.”

8. M., 16. Good health.

9. M., died at 3 of measles.

10. F., died at nine months of measles.

11. M., 9. Good health.

12. F., died at a year and three quarters, of measles.

13, 14, 15, 16, 17, 18. Miscarriages at about four months.

None of the children, according to the mother’s memory, had suspicious symptoms in infancy, but the sixth was the most delicate baby.

Remarks.—We have here a large family only one member of which shows undoubted signs of inherited syphilis, she being born after two miscarriages, which had been preceded by three healthy children. After five others had been born, all of which remained free from symptoms, we have a series of six miscarriages.

XXVIII. *Inheritance from Father.*—Case of Fairlie D—, 8. F. 280.—Husband probably had syphilis before marriage. No evidence that the mother ever suffered at all. Married eleven years.

1. M., a premature birth, died at five days.

2. M., Fairlie D—, 8, kerato-iritis and characteristic physiognomy. Teeth malformed, but not characteristic of syphilis.

3. F., died at one week.

4. Miscarriage.

5. M., living, *æt.* 5.

6. F., died of “wasting” at four months.

7. Miscarriage.

8. M., born dead at seventh month.

Remarks.—The taint has probably persisted through eleven years.

XXIX. *Inheritance from Father, probably.*—C. 313. Case of Alice B—, 8.—Date of syphilis doubtful. Father had a deep ulcer on one arm about three years ago. Mother had eruption of red blotches on her legs after birth of the eleventh child (the patient); these

have left stains eight years after. Thus, probably the mother's disease was due to her foetus.

1. M., died at six years.
2. F., living, 22. No symptoms in infancy or since.
3. F., living.
- 4, 5, 6 and 7. Miscarriage at about third month.
8. M., 16; no symptoms at any time; healthy.
9. F., 15;
10. M., 11;

Probable date of syphilis in father.

11. F., Alice B.—Scaly rash on forehead at three months; subsequently severe symmetrical keratitis, partial deafness, elongation of tibiæ. Teeth good.

12. M., 6. Healthy (not seen).

Remarks.—A single member of a large family suffering from syphilis; predecessors and successor reported healthy. Several miscarriages at a distance from the syphilitic child, and with healthy children intervening. Probably the disease was contracted by one or both parents not long before Alice B— was conceived.

XXX. *Inheritance from Mother and Father, probably.*—L. H. Letters, 1871, 343. Louisa S—, 29.—Mother had sores on genitals before first confinement, but does not remember any rash. Now has ulcers and scarring on one forearm and on front of neck. No history of husband.

1 and 2. Both born dead.

3. Died with snuffles and rash at three months.

4. Living, æt. 5 years. Attended at L. H. for a rash on the face, but no details obtainable.

5. Living and reported healthy and not to have had any symptoms.

6. Died at nine weeks with eruption and other suspicious symptoms.

Remarks.—Here the taint seems to have persisted through the family, but to have fallen on different children with very varying severity.

XXXI. *Inheritance from Father*.—Case of Mary Anne S—. F 356.—Father had some venereal disease when æt. 18, two years before marriage. Was salivated for it, and “afterwards” had ulcers on the left leg for several years, which have left scars. No definite history of secondary symptoms.

Mother died of "decline" at 54.

Family consisted of sixteen children, born alive. Of these only five are now living; eleven are dead; some died at a few days, some at about fifteen months, and one at 26, of "decline,"

The five living children :

1. (the first born), æt. 27; married. No history of symptoms. (Order of the next three doubtful.)
2. F., æt. 21. Married; no history of symptoms.
3. M., æt. 18. No history of symptoms.
4. Mary Anne S—, æt. 15. Well-marked interstitial keratitis of long duration. Other evidence wanting.
5. F. (the 16th and last child), æt. 12. No history of symptoms.

Remarks.—It is possible that in this family the tendency to transmit syphilis extended over many years. This depends upon the assumption that the one who died æt. 26, and who had been under my care for her eyes, was tainted. If she were, then it is certain that a sister, ten years younger, suffered also. On the other hand, the disease may have begun either for the first or second time just prior to the begetting of our patient, Mary Anne.

XXXII. *Inheritance from Father and Mother.*—Case of Wm. M—, 18, G. 318.—Father, formerly a soldier, told the mother that he had had venereal disease several times before marriage, but never admitted having it after marriage. It is probable, however, that he had syphilis during or just before his wife's fourth pregnancy (miscarriage). After this event he was in military hospital for four months for an inflamed eye, while immediately after the miscarriage his wife had well-marked secondary syphilis, for which she was salivated. The history of the children makes it highly probable that the father had had syphilis also before marriage.

1. F., living, 23. Had bad snuffles and thrush.
2. Miscarriage.
3. F., living, 20. Weak health. No infantile symptoms. Now father's syphilis (? second attack).
4. Miscarriage.

Now mother's secondary symptoms.

5. M., Wm. Matthews, 18; history of condyloma (probably) at one year, and about same time severe rash of "round places like burns," "as large as shillings all over him" (? late secondary). Now has interstitial keratitis, typical teeth, and characteristic physiognomy.

6. M., died at three months. "Convulsions."
7. F., died at nine months. "Whooping-cough" and "debility."
8. F., died at one month.
9. M., died at five months. "Measles."
10. F., born dead.

Since the last child there have been two or three miscarriages, also about six or seven between the various children; eighteen or twenty pregnancies in all, and only three children surviving infancy.

Remarks.—The mortality in this family was very heavy indeed.

It is unfortunate that I had no opportunity of seeing Nos. 1 and 3, and making certain whether or not they were tainted. If they were then it is probable that both parents had syphilis twice, the father after chancre-syphilis and the mother after syphilis by conception.

XXXIII. *Inheritance from Father*.—Case of Albert B—, 13, G. 91.—Father had “clap” for three weeks two years before marriage; “cured by three boxes of pills.” Had no secondary symptoms. No note as to mother.

1. M., living, æt. 15. “In infancy covered with sores all over.”

2. F., living, æt. 14. “Well till vaccinated, then all her teeth decayed.”

3. Miscarriage.

4. M., Albert B—, 13. Severe double keratitis; deafness; typical teeth and characteristic physiognomy. Remarkably soft skin. Death at æt. 15½, of “disease of the brain.”

5. M., died at four years. “Consumption;” was very delicate from birth.

6. F., living, æt. 9.

7. Miscarriage.

8. M., born dead.

9. F., living, æt. 4.

10. Miscarriage.

11. M., living, æt. 11 months. Is delicate.

It is very probable that the fourth child suffered from a taint contracted before marriage. The facts are imperfect, because none of the other children were inspected.

XXXIV. *Inheritance from Father and Mother*.—Wm. W—, 7 months. M. F. J., p. 421.—Mother had syphilis from husband a month after marriage. No other details.

1. Born dead.

2. Wm. Wooley, æt. 7 months. Born twelve months after first child. Is under care for well-marked snuffles, syphilitic rash, and sores at angles of mouth.

XXXV. *Inheritance from both Father and Mother*.—Mrs. M—, 26, and child, M. F. K., p. 173.—Mother married twice. During first pregnancy by second husband she had syphilis fully. She was not fully salivated.

The mother's children by first husband are—

1 and 2. Living and reported quite healthy.

Now father's (second husband) and mother's syphilis during pregnancy with—

3. Born dead at seven and a half months.

4. Hannah Elizabeth M—l, æt. ten weeks. Healthy at birth, but

soon began to snuffle, and now has in addition well-marked syphilitic rash.

Remarks.—In this case it would appear that syphilis contracted during pregnancy (exact date uncertain) caused the death of the foetus.

XXXVI. *Inheritance from both Mother and Father.*—Case of Mrs. W—, 33, Priv. H. 344.—Father has had syphilis, but its date is not stated. The mother had chancre and secondary symptoms six years ago, soon after her first confinement. Probably took mercury, but not to salivation. Now has fissures in nostril.

1. Born dead at full time, nine months after marriage.

Now mother's syphilis.

2, 3, 4, 5, 6. All born prematurely at six to seven months, and died a few hours or days after birth. The sixth had spots on skin, and died in half an hour.

Remarks.—It is not improbable that the mother really had her chancre just before her first confinement, and that the foetus died in consequence. The effect of the taint as regards the subsequent conceptions was unusually fatal.

The foregoing series of facts have been taken, with but little selection, from my note books, and they comprise but a small portion of those which I possess. They are sufficient, however, not alone to illustrate some of the difficulties which attend the inquiry, but to furnish valuable evidence on several of the questions under debate. Amongst the difficulties is to be mentioned, first, the circumstance that in most cases the history has often to be collected backwards from the memory of untrained and uninformed observers. It is but very seldom that the whole history of a syphilitic family can be got from a medical witness who has himself been conversant with all the facts. This risk as regards errors in statement, which applies to almost every individual case-narrative, renders it needful to place a large number in juxtaposition, that thus the fallacies of some may be corrected by others.

I shall not attempt any statistical analysis of my facts, since such could not be otherwise than tedious and unsatisfactory. I must leave the reader to form his own impression as to their teaching, and shall base the few remarks which I append in part upon them and part upon my recollections of others.

One of the most important of the fallacies which we encounter is that of—

The possible influence on offspring of second attacks in one or other parent.

Although second attacks of true syphilis are rare, yet they do

undoubtedly occur. That the course of the disease differs much from what is usual in first attacks seems highly probable. In not a few a second infection ends, or appears to end, with the local sore, and in those in which constitutional phenomena are witnessed they are usually mild. Now and then, however, a person who has at a distant period been through a well characterised attack of syphilis passes through a second one, which is complete in all its stages. It is, of course, very common for patients to tell us that they have had the disease several times, and although in the majority of cases we put aside this statement as a mistake, yet it is not safe to do so always. This possibility of a second infection makes it unsafe to measure in all cases the period of liability to transmit from the date of a known attack.

I have endeavoured, whenever possible, to trace the taint to the parent from whom it originated, with the object of trying to answer the question—

Is inherited syphilis more severe when derived from the mother than from the father?

I take for granted (although I know that there are still some who doubt) that it is possible for a father to transmit the taint, the mother being at the time of conception wholly free. I believe, indeed, that in practice this is by far the most common way in which syphilis is transmitted. Whether in these cases it is correct to speak of the inheritance being paternal only is, as we have just seen, another matter, since it is possible that in every instance the mother derives an infection from the fœtus, and may thus in return influence it. It is, however, scarcely probable that an embryo in which the disease existed in such power as to be competent to contaminate the maternal organism, should be susceptible of further hurt in the manner suggested.

It is, fortunately, not common to encounter cases in which married women have had syphilis, either before marriage or subsequently, independently of their husbands.

We have, therefore, comparatively few observations of families born to a tainted mother, the father being free. The foregoing list contains two such, in both of which the mother after marriage was infected through a chancre on the nipple (See Cases VI and VII). In each instance the subsequent children suffered severely, but not more so than in many instances in which the inheritance was paternal only. From time to time other cases of this kind have come under my notice, and although I have formed a general impression that it is rare for the children to escape when the mother has suffered from chancre-syphilis, yet I know of no facts which would uphold

the belief that they do so more severely than under other circumstances.

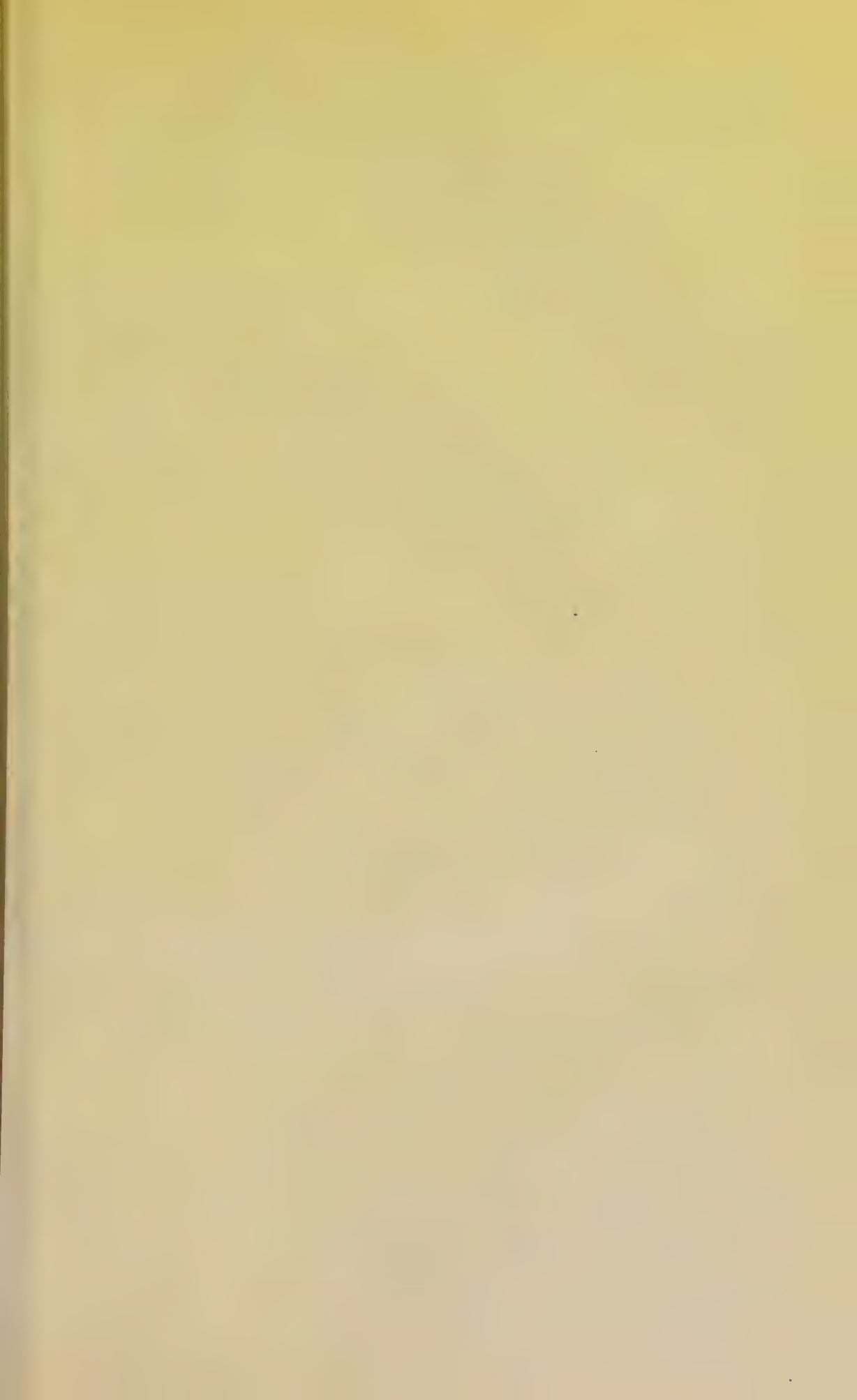
The question, *Is inherited syphilis more severe when inherited from both than from one parent?* must be answered in the same manner as the preceding one, there is no proof that it makes any difference whether the inheritance is from one or both.

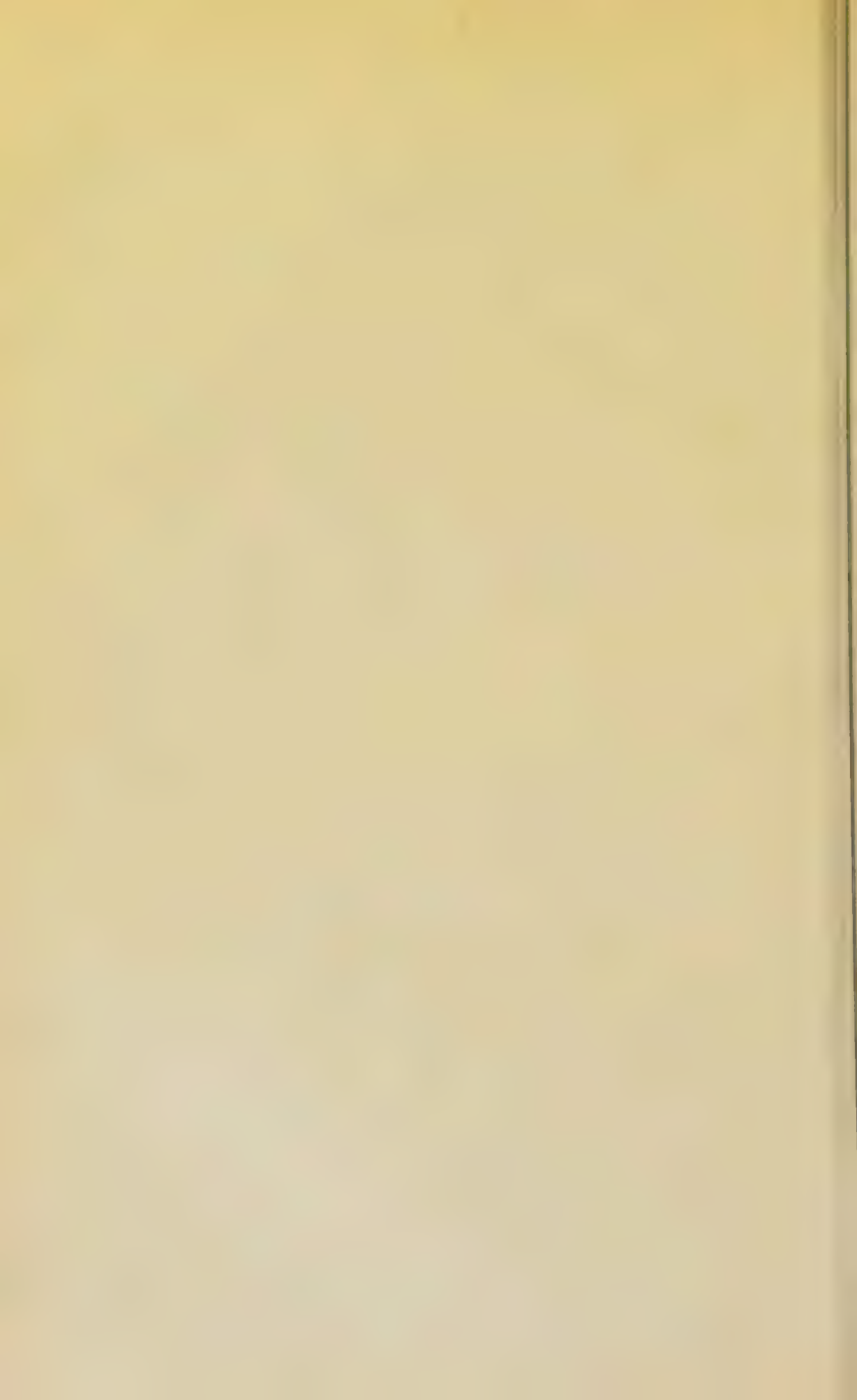
That the noteworthy differences between chancre-syphilis and the inherited disease are to be interpreted by consideration of the differences in the tissues of the growing child and the adult is made very probable by what is observed when a mother near the end of pregnancy becomes infected with primary disease. In such a case the fœtus, nearly full grown, acquires the disease, without a chancre, directly from the maternal blood. It is acquisition, not inheritance, for at the date of conception both the paternal and maternal elements were free from taint, and during the first six, seven, or even eight months of intra-uterine life the fœtus remained healthy. Yet, as I have proved elsewhere by citation of cases, syphilis obtained in this peculiar method resembles exactly that which comes by true inheritance, and not that which follows a chancre. This important fact goes, with many others, in support of the belief that the poison of syphilis remains identical, however obtained, and that the differences which are so patent in its manifestations are due to differences in the state of its recipient.

For the present, I am certainly inclined to believe that we must abandon the old doctrine as to the possible transmission of less or more of taint, and must hold that whenever a child derives the disease by inheritance, it derives it as a whole, and with liability to its full development. In proportion to the distance from the parental acquisition is the probability that the offspring will escape, and hence the frequency with which we see the eldest in the family suffer, and the others escape. If, however, the others escape, they do so altogether, and not with the liability to the development of the disease in slight or imperfect forms. In close connection with this statement comes the question as to whether it is possible for a parent in whom the disease exists to fail to transmit it; whether, for instance, of twins, one may suffer and the other escape, or whether of three successive births one and three may suffer and two escape. Probably it is not possible when syphilis exists in an active form in a mother for her offspring to escape it, but in the case of the father we can by no means feel so certain. That the virus may be present in the semen on one occasion and absent on another is quite conceivable. Here again, however, Colles' law comes in to our confusion, and here again we have to admit and remember the wide range of varia-

bility in the severity of the disease. It is not possible to feel sure that any given child has escaped, for we know very well that all infantile symptoms may have been absent, and the growth and development may have been perfect, and yet at puberty, or even much later, for the first time symptoms unquestionably due to inherited taint may show themselves.

NOTE.—The cases are given very briefly, and are mere abstracts of longer narratives. In most instances I have retained references to their manuscript, partly for my own guidance and partly that I may be able, should it be necessary at any future time, to supply more facts.





ON
PARALYSIS OF THE INTERNAL MUSCLES
OF THE EYE

(OPHTHALMOPLEGIA INTERNA).

A GROUP OF SYMPTOMS WHICH PROBABLY INDICATES
DISEASE OF THE LENTICULAR GANGLION.

BY

JONATHAN HUTCHINSON, F.R.C.S.,
SENIOR SURGEON TO THE LONDON HOSPITAL; SURGEON TO THE MOORFIELDS
OPHTHALMIC HOSPITAL; AND TO THE HOSPITAL FOR SKIN DISEASES.

Read April 9th, 1878.

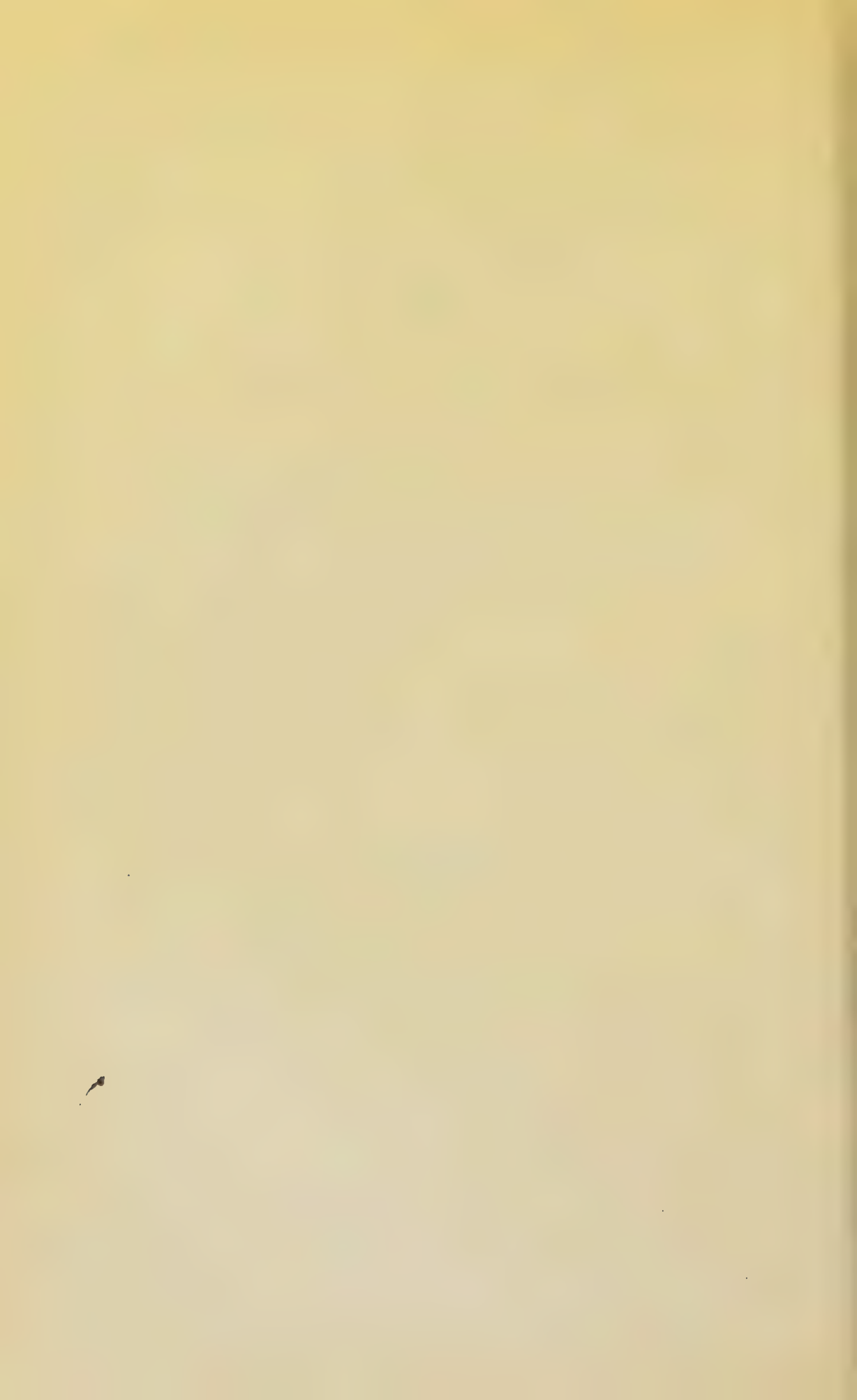
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1878.



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(Received January 8th.—Read April 9th, 1878.)

I AM not aware that any observer has as yet associated any group of symptoms with disease of the lenticular ganglion. Nor am I in a position to prove by an appeal to dissections that any group of symptoms can beyond doubt be so associated. The object of the present paper is, however, to point out that a certain combination of signs would be likely to follow destructive disease of that body; and secondly, to show that we do meet with cases in which precisely this combination is present in the most definite and remarkable manner. All the patients in whom I have seen it are as yet living, and I have therefore had no opportunity for post-mortem dissection.

The lenticular ganglion receives branches from three sources, two of them motor, and one sensory; the third nerve, the vaso-motor, and the fifth. The distribution of the sensory filaments proceeding from the ganglion cannot easily be identified in bedside observation, since the front of the eyeball receives nerve-supply from the other branches of the fifth, and it may be conjectured that no appreciable anæsthesia would result from destruction of the small filaments which pass into the ganglion. The motor roots, however, supply important structures, the iris and ciliary muscle, paralysis of which can easily be appreciated.

The symptoms which *à priori* would be expected to result from destruction of the lenticular ganglion are iridoplegia or paralysis of the iris, both as to its circular and radiating fibres, and cyclo-plegia or paralysis of the ciliary muscle. My suggestion is that when this triad of symptoms is present—inability to use the ciliary muscle, paralysis of the dilating fibres of the iris, and paralysis of the contracting fibres of the iris,—without any other form of orbital paralysis, the seat of disease can be in no other structure than the ganglion itself.¹ The diagnosis depends equally upon the presence of all three, and the absence of all others. If for instance any of the recti muscles are affected also, we are at once carried further back to the trunk of the third or sixth nerve; and if only one or two of the separate symptoms forming the triple group were present, the inference would be ready that the disease might be seated either in front of the ganglion or behind it in the trunk of the nerve involved. The ganglion itself is the only place where the two motor nerves (third and vaso-motor) are in contact, and where both may be affected by disease which does not involve any other parts.

¹ My only hesitation on this point is as to whether in some cases the same symptoms may be due to disease near the nucleus of the third nerve. It is not easy, however, in such cases to see why the vaso-motor should be affected. I have, however, seen the pupil motionless and accommodation lost in some cases in which the disease was believed to be in this position; always, however, there were other complications.

PARALYSIS OF THE INTERNAL MUSCLES OF THE EYE.

Clearly, then, it is easy to state beforehand what are the symptoms that ought to follow destructive disease of this ganglion, and to see that they could scarcely be caused by disease located anywhere else.

The second part of my task is to show that there are cases in which this precise group of symptoms is met with, and afterwards I shall have to make some suggestions as to the probable nature of the disease in question.

Of the three structures named it is probable that the circular or constrictor fibres of the iris are supplied solely by the third nerve, that its dilator or radiating fibres are innervated solely by the vaso-motor, and that the ciliary muscle although perhaps dependent in some degree upon both nerves for perfection of function, is chiefly supplied by the third. Any one of these muscular structures may be paralysed alone, and although combinations sometimes occur which it is difficult to interpret,¹ yet it is beyond doubt that the vaso-motor nerve may be wholly paralysed without cycloplegia, whilst the latter is a usual concomitant of dilated pupil due to paralysis of the third nerve.

The first case which drew my attention to this subject occurred in 1865, and was one of the most important that I have seen because the disease was slowly aggressive, and I was able to watch its several stages. The patient was a clerk (Mr. C—), æt. 34, when he first came under care. In the *left* eye the pupil was contracted owing to complete paralysis of the radiating fibres of the iris; he also had difficult accommodation with the same eye. He remained under my observation till 1868; at which date in the *left* eye iridoplegia was complete, and accommodation wholly lost,

¹ Although probably by far the greater part of the motor-nerve supply to the iris and ciliary muscle passes through the ganglion, it is not certain that these structures are wholly dependant on that body. Experiments in dogs make it seem probable that some vaso-motor twigs to the dilating fibres of the iris pass with the long ciliary, and never enter the ganglion. It may be so in man, and, within limits, the anatomical conditions may possibly vary in different individuals.

whilst in the *right* there was complete iridoplegia, but accommodation was still good. There was a remote history of mild syphilis, but although iodide of potassium was given no benefit followed, and the symptoms slowly advanced.

My next case exactly resembled the early stage of the one just recorded, but my notes do not extend over any long period, and I cannot therefore assert that the loss of accommodation ever advanced quite to completeness.¹ The patient, a gentleman, æt. 35 (Mr. E—), came under my observation in April, 1867. In his *left* eye the pupil was absolutely motionless and accommodation much weakened; with a + 15 lens he read No. 1 Jaeger easily. The other eye was perfect. He appeared to be in good health and denied all history of syphilis. He had suffered severely from sciatica. None of the recti muscles were defective. In a bright light the pupil of the paralysed eye was a little larger than that of the other, but in the shade this relation was reversed, the paralysed pupil remaining stationary while the other dilated.

CASE 3.—In April, 1869, a woman (Mrs. T—), æt. 44, who had been under my care for primary and secondary syphilis in 1865, but had in the interval been quite well, came to Moorfields on account of sudden failure of sight in both eyes. Her pupils were of moderate size, and so extremely sluggish that I was in doubt whether it could be said that they acted at all. She assured me that until three weeks before she could see to thread her needle perfectly, and that the loss of ability to do so had been quite sudden. With + 16 glasses she could just manage to read No. 1 Jaeger; her distant vision also was not quite perfect, in consequence perhaps of some small films which were present in the vitreous of each eye.

CASE 4.—*Complete symptoms of disease of lenticular ganglion in left, partial irido-plegia with weak accommodation*

¹ This case has been published in rather more detail in vol. vi of the 'Ophthalmic Hospital Reports,' p. 142.

in right.—Elizth. B—, an unmarried servant girl, who furnished the subject of this important case came under notice in June, 1873. In the *left* eye there was complete paralysis of the iris and ciliary muscle, whilst in the *right* the pupil was very sluggish, but the ciliary muscle was unimpaired. Not the slightest movement of the left iris could be provoked even by exposure to the strongest light. With a + 7 glass she read 1 Jaeger easily. It was probable from her statements that the symptoms had been slowly advancing for six months. She had suffered from much pain across the forehead. I did not ask the direct question as to syphilis, and I failed to detect anything about her in support of such a suspicion. Although in the *right* eye her power of accommodation seemed for the present nearly perfect, she said that the eye soon tired; and it was her impression that it was failing just as the other had done. This patient attended at Moorfields from June 19th to Oct. 2nd, and took throughout iodide of potassium in five grain doses. At the latter date she had so far improved as to be able with the *left* (paralysed) eye to read No. 4 Jaeger, although not easily. The pupil of this eye was still rather larger than that of the other (in a bright light). No further evidence had been obtained as to its possible cause.

CASE 5.—My fifth case is that of a gentleman, æt. 35 (Mr. T—), who discovered that his *right* eye was failing in 1861.

He consulted an eminent ophthalmic surgeon; his symptom being a certain inability to read whilst vision for distant objects remained good. This eye never regained its power of accommodation, and he was obliged ever afterwards to depend wholly upon the left for reading. Some time later the right eye began to diverge, and for troublesome diplopia he came under my care in April, 1869.

I found his *right* eye slightly divergent, its pupil absolutely motionless, and the function of accommodation lost. With a + 12 glass he read No. 1 Jaeger easily with the affected eye, and the sight of this eye for distant test-types was perfect without a glass. It appeared that the superior,

inferior, and internal recti were all in a slight degree weakened. His other eye was not in any way affected, except that it presented a low degree of myopia. He denied any history of syphilis, and I could detect nothing suspicious. His face was covered with acne. He was subject to "rheumatic aching in the limbs." He had suffered from certain other nervous symptoms, thus, he was liable to a nervous cough which sometimes produced vomiting; he often had severe sick headaches; about a year ago he suffered an attack of most violent neuralgia in the face lasting a few days. These were the only items of evidence I could get in reference to his predispositions. It will be seen that the symptoms had lasted in the one eye for eight years, and that the other had not suffered in any degree. It appeared not improbable that disease beginning in the ganglion had travelled backwards to the trunk of the third nerve.

CASE 6.—Mr. H—, a veterinary surgeon, æt. 36, was sent to me on June 2nd, 1875, from a town in the midland counties. In him the condition was symmetrical, and in both eyes paralysis of the iris was complete and accommodation very feeble. He had had syphilis eight years before, and the details of his attack and treatment were supplied to me by the surgeon who attended him. He had no other symptoms of nerve disease excepting constipation, flatulence, and occasional stabbing pains in his limbs. I treated him for several months but without being able to prove any benefit. Sluggishness of pupils and occasional failure of sight had been noticed for a year before Mr. H— came under my care. When I first saw this patient he read 20 Snellen at twenty feet, but could only read large print without glasses. His optic discs were quite healthy, and his sight with low convex glasses was perfect for near objects. My notes do not state the precise degree of failure of accommodation, but at a subsequent visit he saw No. 5 Jaeger with one eye, and No. 8 with the other. His reason for applying to me was that he could not see to read or write efficiently. The cycloplegia was, however, neither complete nor constant;

thus, at one of his visits to me he managed with great effort to puzzle out a few words of No. 1 Jaeger. This was after some months' use of mercury and iodine, and after he had somewhat improved. His pupils remained throughout absolutely motionless; I tested them in sunlight and used a lens. The stabbing pains mentioned above had been severe enough to suggest the possible commencement of locomotor ataxy, but there were no symptoms conclusively indicative of that malady. He compared the pains to the pricks of a knife, and said that formerly they had occurred chiefly at night and would often keep him awake. His lower extremities were the parts usually affected. He got rid of these pains under iodide treatment, and is now, I believe, quite well.

Hitherto I have not been able to record in any single case that a cure has occurred. In the following instance, however, it seems highly probable that one of the three functions has been regained, since "paresis of accommodation" had been diagnosed four years before the patient came to me, and from this he had almost wholly recovered when I saw him, the iridoplegia, however, remaining complete.

CASE. 7. In which twelve years after syphilis the ciliary ganglion of one eye was affected, and recovery of the function of accommodation with permanent iridoplegia resulted.—This case is one of especial interest because I am able to feel certain as to the patient's antecedents, and also to state the result of treatment. The patient came under my care in May, 1871, and again on Oct. 8th, 1876.

Mr. S— was treated by Mr. H. J. Johnson, in 1860, for chancre followed by eruption, &c. He remained fairly well until 1870, when he was under Mr. Buxton Shillitoe's care for ulcers in the throat and on the tongue. At this time he also became deaf, and consulted Mr. Harvey. In May, 1871, he was brought to me by his medical attendant, Dr. Reilly, of Bow, on account of intractable syphilitic ulcers on the palate and tongue. I saw him only once, and he was subsequently under the care of different surgeons both in England and America for the same malady, always affecting

the same parts. In Oct., 1876, he returned to me having just come home from the Philadelphia Exhibition, and still suffering from sore tongue and lips. He now told me that his *right* eye had suffered about a year after his first visit to me, and that he had been under the care of one of my Moorfields colleagues on account of it. I found his *right* pupil of the same size as the other, but quite motionless whilst the left acted well. He could see almost perfectly in the distance with the *right*, but could not read so easily nor at so short a distance with it as with the other. His near point was eight inches with the *right*, and five inches with the *left*. He told me that when he applied to Mr. H— he could not see to read with the right eye at all though he could see with it in the distance, and that under treatment the sight of this eye had gradually improved. He considered that the eye was now quite well, and was not aware that the pupil was motionless. Mr. H—'s treatment had consisted in a six months' course of five-grain doses of iodide of potassium and the use of Calabar bean. I was shown Mr. H—'s hospital letter upon which "paresis of accommodation" was written as diagnosis; and on it was recorded that after the use of Calabar bean his near point with the *right* was eight inches, and that of the *left* five inches. It will be seen that we have no measure of the degree in which accommodation had failed, but Mr. H—'s expression in the note and the man's memory of his symptoms may be taken as evidence that the function was very considerably reduced.

Looking at the fact that the pupil is now motionless and neither contracted nor dilated, we may take it as highly probable that in the first instance all three functions were lost, and that the accommodation alone has been benefited by treatment. He has had no other symptoms of syphilis of the nervous system.

General summary.

Whatever may finally be thought as to the recognition of the precise part of the nervous system affected, it is apparent

that we have in the cases described in this paper illustrations of a very curious group of symptoms. The cases for the most part very closely resemble each other. In none of them was the patient very seriously ill, and in only two were there indications of disease of other parts of the nervous system. In none did the disease of the nervous system show any tendency to extend whilst the patients were under my observation, a fact, however, which may perhaps have been partly due to their having been all treated by specifics. Of the eight cases, in five both eyes were affected.¹ In three of the eight there was no history of syphilis (two men and one woman), but in none of these can the absence of history be held to prove absence of the reality. In two of these three only one eye suffered, the disease in one of them having been present eight years without showing any tendency to attack the other eye. All my patients were at an age when syphilitic affections of the nervous system are common; the oldest was forty-four, and the youngest twenty-seven when the disease began. In one case I had myself attended the patient for severe syphilis four years before the eye symptoms, and in four others there was the definite history that symptoms of constitutional syphilis had occurred a few years ago. I cannot but regard it as highly probable that in almost all cases the affection is due to syphilis.

We may note that it does not appear to occur amongst the later forms of tertiary disease, but rather during that period in which after disappearance of secondary symptoms there is usually a long period of latency.

One fact I must ask especial attention to; it is that the paralysis of the iris appears to precede the loss of accommodation, and also to resist treatment longer. In four cases it is expressly stated that the iridoplegia was first developed, and in two of them the patient regained to some extent the use of the ciliary muscle. In none was the failure of accommoda-

¹ As regards symmetrical occurrence the facts are very similar to what we find in the case of syphilitic choroiditis, in which frequently both eyes are affected, sometimes only one, and but rarely both in quite equal severity. The two conditions occur also at almost the same periods after the primary disease.

tion the first symptom, a fact the more important when we consider that it causes a condition which the patient is not likely to overlook, whereas a motionless pupil may easily escape attention. It is, I admit, difficult to explain, on the theory that the disease is in the ganglion, why the ciliary muscle and the sphincter of the pupil should suffer in different degrees.¹

It has been necessary in dealing with this somewhat intricate subject to use a number of terms some of which can scarcely be said to be in common employment. It may be convenient, even at the risk of some repetition of what I have just said, to define these and to add a few words about the conditions severally designated by each.

Paralytic mydriasis is the condition of dilatation of pupil which occurs when the circular fibres are paralysed and the dilators intact. It may result from disease in the short root of the ciliary ganglion, or in the trunk of the third nerve, or in the brain itself. When present alone it may be presumed to be due to disease in the short root. As an uncomplicated condition it is very rare.²

Paralytic myosis is the condition of contraction of the pupil which occurs when the radiating fibres are paralysed, the circular ones remaining intact. It results from disease of the vaso-motor root of the lenticular ganglion or from disorganisation of the trunk of the vaso-motor nerve in the neck, or from disease or injury to the brachio-cervical region of the spinal cord. It is seldom in such a case that the pupil is contracted in any marked degree, unless the circular fibres are thrown into spasm: a condition of simple inability to dilate but without any special contraction is the common result of paralysis of the vaso-motor. As a consequence of

¹ Is it not reasonable to suggest that the disease of ganglion acts at first by simply weakening the nerve currents which traverse it, and that a current which may be too weak to reach to the iris may still get as far as the ciliary muscle?

² When mydriasis occurs with cycloplegia, and without other complication, it may be conjectured that the third-nerve root of the ganglion is diseased. I have notes of several cases in which this diagnosis was given. I have never seen both eyes affected.

injuries to the cord or to the sympathetic trunk in the neck paralytic myosis is not uncommon, and is usually combined with narrowing of the palpebral fissure and retraction of the eyeball. When not so complicated it may probably be due to disease close behind the ganglion.

Irido-plegia is a term applicable to total paralysis of the iris, both of its circular and radiating fibres. It is usually an indication of disease in the lenticular ganglion, and is then commonly combined with cycloplegia. It may at times occur from disease in the centres, but when such is the case other combinations might be expected.

Cycloplegia denotes paralysis of the ciliary muscle, and is expressed by loss of power of accommodation. In young and middle-aged persons it is easily appreciated, the necessity of a strong convex glass for reading (+ 10 or + 12) being conclusive. Cycloplegia occurs often, without combination with other ocular paralysis, after diphtheria, but in all other conditions it is rare excepting with paralysis of the iris. It does not occur, or at any rate is not complete, when the vaso-motor trunk in the neck is paralysed. We may presume that the ciliary muscle is supplied almost solely by the third nerve, but perhaps partly by the vaso-motor.

Ophthalmoplegia interna is a term which may perhaps be conveniently used to indicate a state of paralysis of all muscular structures within the eyeball (both sets of fibres in the iris and the ciliary muscle), and excluding all the external or orbital muscles. It is the symptom, or combination of symptoms, with which the present paper is concerned, and when it is present without any addition I claim that in all probability it denotes disease located in the ciliary ganglion.

A few words, in conclusion, on the general bearings of the subject may perhaps be permitted. Pending the results of post-mortem dissection it must still remain doubtful whether the group of symptoms which I have described is due to disease in the peripheral ganglion or in some central nucleus the position of which is not yet known to anatomists. If we accept, as I think we may safely do for the present, the

hypothesis that the ganglion is its site, we have, then, tolerably conclusive evidence that it is possible for syphilis (and perhaps other causes of disease) to attack and disorganise the separate ganglia connected with the vaso-motor system. If the lenticular may be thus involved why not Meckel's or the otic, why not any one or any pair of the cervical or thoracic ganglia? Our task in the future must be to seek out and identify the groups of symptoms which diseases of each of the ganglia separately or of many together would produce, and it may easily be that here we shall find an explanation of some symptoms which have as yet been regarded as anomalous.

Another line of investigation in the future will be as to whether when disease has begun in a peripheral ganglion there is much tendency for the morbid process (probably a neuritis) to travel along the nerves and involve other trunks or even reach the central nuclei. In two of my cases something of this kind appeared to have happened, for the patients who began with ophthalmoplegia alone had subsequently paralysis of the third. In none others was there any tendency shown to invade other nerves, but as already stated it must be remembered that all the patients were treated with specifics. There is another group of cases which I hope to have the honour at an early opportunity of bringing under the notice of this Society in which all the external muscles of both eyeballs become either in succession or simultaneously partially paralysed, and for which I shall venture to propose the name *ophthalmoplegia externa*. This affection is usually due to syphilis, and in it probably the centres are affected. It curiously and unexpectedly happens that in it the *internal* muscular apparatus of the eye often escapes. In one case, however, which is included in the appended table, the internal paralysis in a complete form preceded that of the external muscles, but as a rule it is remarkable that the two affections, although due to the same cause, keep distinct.

A contracted and sluggish pupil is a well-known symptom of locomotor ataxy, and often an early one. It is probably caused by defect in the vaso-motor fibres, and is not usually

attended by failure of the ciliary muscles. In one or two of the cases which I have mentioned symptoms were present which suggested the possibility that ataxy was threatened, and in connection with this subject we must recollect that several French observers, M. Fournier most prominently, have adduced facts in favour of the belief that this malady is in many cases due to syphilis. My own experience quite supports M. Fournier's statements, but it is to be remembered that to both of us probably only special forms of ataxy have been presented. Whether or not ophthalmoplegia interna is often met with in association with ataxy must be left for further investigation, but the facts as at present known would not favour such a suspicion.

I must finally add a word of apology for the endeavour to introduce a new term into use. I am well aware of the inconveniences of a loaded nosology, but as I feel confident that the group of symptoms described is a definite and important one I feel justified in suggesting for it a distinctive name. Whether this course is desirable and whether the proposed name is a good one must be left to the decision of the future.

A tabular statement of the cases which have occurred in my experience is appended overleaf.

Table of cases of ophthalmoplegia interna.

No.	Name.	Age.	History as to syphilis.	One or both eyes.	Eye first affected.	Remarks.
1	Mr. C—,	34	Mild syphilis some years before	Began in one and afterwards affected both	Left	Iridoplegia preceded cycloplegia in each eye. (Notes extend over two years.)
2	Mr. E—,	35	No history of syphilis	One	"	Iridoplegia preceded cycloplegia. (Only a single observation.)
3	Mrs. T—,	44	Syphilis four years before	Both	Simultaneous	Iridoplegia and cycloplegia coincident.
4	Elizabeth B—, single	27	No history of syphilis	Left complete, right threatened	Left	Slight improvement under a course of iodide.
5	A gentleman	35	Ditto	One only	Right	The condition had persisted eight (? 18) years. Some weakness of third-nerve recti.
6	Mr. H—, Veterinary Surgeon	36	Had had syphilis eight years before	Both	Uncertain, perhaps simultaneous	Iridoplegia complete; cycloplegia partial and somewhat variable under treatment.
7	Mr. S—,	37	Had suffered severely from syphilis	One only	Right	Recovered from cycloplegia, the iridoplegia remaining.
8	A sailor	...	Syphilis mildly years before	Both	Simultaneous	With paralysis of all the eye-muscles. The lenticular paralysis had probably preceded that of the recti by several months. (I have not mentioned this case in the body of the paper.)



NARRATIVE
OF A
CASE OF TRUE LEPROSY,
IN WHICH
COMPLETE RECOVERY HAS TAKEN
PLACE.

BY
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ROYAL LONDON OPHTHALMIC HOSPITAL.

Read February 11th, 1879

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1879.

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(Received December 10th, 1878—Read February 11th, 1879.)

THE subject of the appended narrative has been under my observation at different times during the past twenty-seven years. When I first saw her she was suffering from leprosy in a severe and well-characterised form. It was the mixed kind, partly tubercular and partly anæsthetic. She has now regained good health, and although in the state of her right hand, in which ulnar paralysis exists, in the dusky tint of her skin, and in its loss of sensibility in various parts, we have permanent consequences of the disease, yet there have not been for many years any indications of tendency to relapse. All tubercles have long since disappeared, and both as regards skin and nerves, we may consider that reparation has taken place as far as it was possible. As an instance of proved recovery from a disease which is usually steadily aggressive, and especially because I am able to produce the

patient for inspection, I think the case not unworthy of the attention of the Society.

It will be seen that the disease was acquired by an immigrant into a leprosy district who did not inherit any proclivity, and that the recovery took place after a permanent return to England. The patient, during her residence abroad, had been exposed to no hardships, and her case seems to me to lend some support to the opinion which I have long held that the malady is due solely to diet, and that its special cause is probably fish.

Case of recovery from severe leprosy acquired twenty-eight years ago in Jamaica.—Mrs. Harriet I—, æt. 71, was born in Portsmouth, of Jewish parents who had been both born in England. When thirty-two years of age she went to the West Indies, being in excellent health at the time. She lived at Montigua Bay, Jamaica, eleven years, and during the greater part of that time enjoyed good health. Her diet was beef, turtle in all forms (at sixpence the pound), fish and vegetables. The first was plentiful and good. She thinks she took fish almost every day, but adds, “I never took any fish that would injure me—barracouta, or anything like that.” About a year before she returned to England she began to suffer from leprosy. The first local ailment that she remembers was a swelling on the right little finger and within a month or two blotches came on her forehead. It was diagnosed on the spot as “something of the nature of leprosy,” and she was advised to return to England.

On arriving in England (æt. 44) she at once presented herself at the Hospital for Skin Diseases, where, under Mr. Startin’s care, I often saw her. We took great interest in her case as a well-marked example of true leprosy. Her face was covered with dusky tuberculous folds of thickened skin. Her arms also showed many patches, and were in parts anæsthetic. She got no benefit from our treatment, and after ten months’ attendance left us, and obtained admission into Guy’s Hospital under the care of Dr. Addison, who, she says, wished to have a wax model made.

She remained in Guy's Hospital three months, where, she believed, an attempt was made to salivate her, but without success. She next went under Mr. Hunt's care, who pushed a course of arsenic, and, she thinks, made her eyes sore. After this she was under care at Moorfields (Mr. McMurdo) for her right eye, and has continued to attend there almost ever since.

As regards the cure of her skin she states that all drug treatment had seemed to fail and she had left it off. About two years after her return from Jamaica, the skin being still much in the same condition, the return home of a wealthy relative placed good wine within her reach, and she took freely of port and champagne, under which rapid improvement ensued, and in about a year she thought herself well. She has had no return in the skin, and "has never had a day's ill health since; not to speak of."

Mrs. I— is now a very young-looking woman for her years (71), florid, and healthy. She still shows various conditions which have resulted from the leprosy. Her skin, although nowhere quite anæsthetic, excepting in the region of the right ulnar nerve, is in many parts deficient in sensation. Her right eye is inflamed and the cornea ulcerated, with deposit in its structure and much persistent conjunctival congestion. The state of her eye is that characteristic of leprosy, but there have not for many years been any aggressive changes.

As regards the frequency of recovery from conditions of advanced leprosy, it is probable that they are infrequent, and that they occur only under conditions, such as those present in this instance, of complete change of place of residence. I have never myself witnessed a similar case, nor do I know of any which have been authenticated in English practice. In former times many Norwegians suffering from leprosy emigrated into the United States, and I believe that it was thought that a certain number recovered, but I am not aware that this fact has ever been proved by the citation of individual cases. It is, indeed, not often that a patient remains sufficiently long under the observation of the same

surgeon for him to be able to speak definitely of the first and last state of such a protracted malady. I have seen during the last twenty years probably about a dozen cases of leprosy in Europeans. Two of them I know to be dead, but the others have passed from my observation. In every instance I have forbidden the use of fish as the first essential in treatment.

ON

OPHTHALMOPLÉGIA EXTERNA

OR

SYMMETRICAL IMMOBILITY (PARTIAL)
OF THE EYES, WITH PTOSIS.

BY

JONATHAN HUTCHINSON, F.R.C.S.,

SENIOR SURGEON TO THE LONDON HOSPITAL; CONSULTING SURGEON TO THE
ROYAL LONDON OPHTHALMIC HOSPITAL; AND SURGEON TO THE
BLACKFRIARS HOSPITAL FOR SKIN DISEASES.

Read February 11th, 1879.

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1879.

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THE cases to which I have to ask the attention of the Society are characterised by a very peculiar group of symptoms. Drooping of the eyelids, so as to give to the face a half-asleep expression, is usually the first, and it is soon accompanied by weakness of all the muscles attached to the eyeball, so that the movements of the latter become much restricted, or even wholly lost. The condition is usually bilateral, though it is not always exactly the same in degree on the two sides. Its symmetry probably denotes that it is of central origin. It by no means always happens that all the ocular muscles are alike affected, or that they are attacked simultaneously, still it is a very marked feature of the malady, that the muscles fail in groups, and not singly. Non-symmetrical paralysis of single ocular muscles is, of

course, very common, especially in connection with syphilis and locomotor ataxy, but such cases are to be distinguished from those which I am now describing: first, by the fact of non-symmetry: secondly, by the early completeness of the paralysis; and, thirdly, by the ease with which, very frequently, they are cured. In a majority of them, there is, perhaps, good reason to suspect that a gumma in the nerve trunk is the cause. In the symmetrical cases now under consideration, however, the changes probably begin centrally; they are usually slow in progress, and are often difficult of relief. They agree with the single nerve cases in that they occur chiefly to those who have had syphilis. Although I have ventured to speak of immobility of the eyeballs, I by no means wish to imply that it is usually complete. On the contrary, incompleteness in the degree of paralysis is almost as marked a feature as is the tendency to affect many muscles at the same time. Although the eyelids droop, there is seldom complete ptosis; great limitation of the range of motion of the eyeballs is more common than fixation. The degree, however, varies with the stage, and at a late period the paralysis may be absolute. The third, fourth and sixth nerves are of course those which are involved, but not unfrequently in the early stage one or more of these may wholly escape. Occasionally the optic nerve itself is involved, and sight is lost. I am making these statements from the observation of only a limited number of cases, for the condition is but seldom seen. I have as yet witnessed only a single fatal case, and in it evidence was afforded of extension of disease downward into the motor tracts of the cord, and the man lost the use, first of his upper, and subsequently of his lower extremities.

In a former paper¹ I have ventured to propose the name *Ophthalmoplegia Interna*, for cases supposed to depend upon disease in the lenticular ganglion, in which the internal muscles of the eye (the iris and ciliary muscles) are together involved in paralysis. If in contradistinction with these, I may now be permitted to suggest that of *Ophthalmoplegia*

¹ 'Med.-Chir. Trans.,' vol. lxi, p. 215.

Externa for those in which the external muscles of the eyeball fail in power, it must be with the full admission that these latter often, indeed usually, include the internal muscles also. With paralysis of the third nerve, the circular fibres of the iris and the ciliary muscle must of course fail, but the defect in these parts in this class of cases is, usually, as I have asserted of the recti and levators, incomplete. In this incompleteness, at any rate in the early stages, it differs from what we see in cases of supposed disorganisation of the lenticular ganglion. Nor does it always happen, even when in ophthalmoplegia externa the third nerve root to the ciliary ganglion is involved, that the vaso motor filaments suffer too, and in this we find a further difference from what would be expected if the ganglion itself were attacked.

The cases in question are probably closely allied in nature to what is known as progressive muscular atrophy, their peculiar feature being, that only one special set of muscles (or rather nerves) is at first attacked. We have, probably, in them, a very close parallel to the so-called bulbar paralysis, the labio-glosso-laryngeal paralysis of Duchenne. In it, as in ophthalmoplegia externa, central degeneration-changes occur, and the result is the paralysis of a set of associated muscles. It may be plausibly conjectured, that the initial lesion is inflammation of the nuclei of the affected nerves, which, in a slowly serpiginous manner, creeps from place to place, along certain definite anatomical paths. Within certain limits its directions of spreading, and its progressive tendency may vary in different cases, but speaking generally, the cases are remarkably same in their features. In exceptional instances, definite symptoms of locomotor ataxy are present, and in others still more rare, the fifth nerves, or the seventh, or even the eighth may be involved. Although, so far as I am aware, this group of cases has not as yet found any record in English medical literature, it has not escaped notice in Germany. Graefe described examples of it in 1867, and employed the word ophthalmoplegia in reference to them, and Eulenberg in his work on 'Functional Nerve Disorders' devotes a short chapter to the subject, which, however, con-

sists chiefly of an abstract of Graefe's communication. By both these authors it is spoken of as sometimes syphilitic, and sometimes rheumatic, and occasionally without assignable cause.

The clinical part of my paper consists of the narrative of fifteen examples of the malady. Several of these cases extend over many years, and are very detailed. I have done my best at abbreviation, but am very conscious that I shall still need the Society's indulgence in this matter.

CASE 1.—My first case occurred in 1862, and has special interest, because I am able to produce a photograph of the patient. A printing-house porter, aged 52, applied at Moorfields almost blind with white atrophy of the optic nerves. His eyelids drooped a little on both sides, and he was obliged to elevate his head, and use the occipito-frontales in order to see. The eyes were almost fixed in the orbits. The photographer to whom I sent him, reported him "an unusually good sitter, for he never moved his eyes." Both eyes diverged, the left especially. On both sides all the third nerve muscles were paralysed completely, excepting the levator. On the left side, the fourth nerve was paralysed, and the only muscle which he retained was the external rectus. On the right side, the sixth nerve was paralysed, and the only muscle which he retained was the superior oblique. In both eyes the discs were white and the retinal arteries small. He was not deaf, his faculties, excepting sight and smell, were good, and he had excellent health. He had never had "a fit" nor any limb paralysis, and he had never suffered from pains in his head.

The case was remarkable on account of the long interval between the paralysis of the muscles, and the failure of sight. The progress of symptoms in the early stages appeared to have been very slow. The history as to syphilis was that in 1834 (that is thirteen years before his first eye-symptoms) he had a long treatment by mercury for a chancre with buboes. He was salivated. No secondary symptoms were remembered. In addition to the eye-symptoms his smell had failed.

Ammonia made him sneeze, but he could detect no odour in hyoscyamus. He considered his taste perfect.

This man remained under my observation from January, 1862, to July, 1864. In August, 1863, he began to complain that fluids in swallowing came through his nose. His speech was thick, and the palate appeared very deficient in sensation. His pupils were quite motionless, and of medium size. His uvula could not be made to move. In July of 1864 he was in the same state. The left external rectus, and the right superior oblique, still acted well. The muscles of his palate did not act, but tickling it, although but little felt, caused sickness. His cheeks hung loose as if partially paralysed, but nothing positive could be proved. He could still whistle.

After 1864 this patient ceased to present himself. From inquiries at the workhouse I believe that he died in 1866.

CASE 2.—My second case is that of a young physician who, in 1871, put himself under my care for symmetrical paralysis of the third nerves. He was thirty years of age, and had been treated seven years before for a chancre (by the late Mr. Gascoyen). No secondary symptoms followed, but he had since had frequent reminders in the form of psoriasis palmaris, &c. He had also been liable to "rheumatic pains," which were always relieved by iodide of potassium. About a week before I saw him he began to be troubled with morning sickness, then his eyelids felt heavy, and next they drooped, and he began to see double. When he came to me he had slight ptosis on right and complete on the left. On the right the pupil was dilated and fixed, and accommodation lost; whilst in the left the pupil was normal and accommodation was perfect. The internal rectus was very defective on both sides. On the left the superior and inferior recti were almost wholly paralysed, whilst the superior oblique was perfect. On the right these recti were weak, but only to a slight degree, but a fortnight later the ptosis on this side also became complete, and all the recti, excepting the external, were absolutely paralysed. Both eyes diverged, and, on the right side, he had entire loss of accommodation,

At this period he complained much of "horrible muscular pains" in his legs, which were relieved by exercise. He had also morning sickness, almost exactly like that of pregnancy, and some frontal headache. Subsequently there was general dulness of sensation in the lower extremities. Dr. B. remained under drug treatment almost continuously for two years and a half. We gave mercury in various forms, and the iodide in very diverse doses. Sometimes, after an interval, five-grain doses produced very definite results, and at one period he had got up to *an ounce and a half in the twenty-four hours* without feeling any discomfort beyond lassitude. The sum of our experience was, that the iodide did more good than mercury, but that it was useless unless the dose was frequently and liberally increased. The functions of accommodation and the use of the internal recti varied much from time to time, and always afforded us a good test of the efficiency of treatment. I never, in any other case, pushed the iodide to anything like the extent to which it was given in this instance. The result, however, was most satisfactory. Dr. B. lost all his ataxic symptoms, he regained perfect accommodation, and most of the ocular muscles recovered their power. He is now, seven years after the conclusion of treatment, in good health, and has never been threatened with any relapse. It should be added that we had the advantage of sea-air at the time that the largest doses were given, and without its aid I much doubt whether the result would have been as satisfactory as it was.

CASE 3.—The following case was under my observation from its commencement, in 1869, to its end, in 1876, and it is of especial value as the only one in which I have obtained a *post-mortem*. The symptoms, which began with ocular paralysis and amaurosis, were slowly progressive, and the case in part resembled locomotor ataxy, and, in part, progressive muscular atrophy.

Robert S—, æt. 48, a very healthy-looking man from the country, formerly a policeman, now a gardener, was sent to me by my late colleague, Mr. Dixon, in March, 1869. He

had a slight defect of his right sixth nerve, and could not abduct the globe well. There was slight convergence, and he complained of seeing double. The right pupil was rather larger than the other. He complained that he did not see well by artificial light, and that he could not read long at a time. He had enjoyed good sight until six weeks before, and had never used glasses. I found that without glasses he could, by effort, with left eye read No. 10, but with right only 14. For a minute or two he could puzzle out smaller print, and it appeared that his defect was wholly due to weak muscles of accommodation, for with +16 glass he read No. 1 easily with either eye, and his distant vision was almost perfect.

I could not arrive at any opinion as to the cause of the rather sudden onset of symptoms. He denied having ever had syphilis, and he had been married early and had seven living children. He had never had any injury to his head more serious than the blows to which policemen are liable, nor had he had gout. Of late he had suffered a good deal from what he called "rheumatism" between his shoulders and had also had giddiness and much pain across his forehead. I prescribed iodide of potassium, and during a six weeks' treatment with it he made some improvement, and I then lost sight of him. I am sorry that I cannot state the exact degree or kind of improvement, but he became able to continue his work as gardener with comfort, and, as he lived at a distance, he was anxious to avoid journeys to town.

Four years later, in October, 1873, Robert S— again came to me at Moorfields. In the interval his sight had steadily deteriorated, and with the right eye he had now only bare perception of light, whilst with the left he could scarcely spell No. 20. He could not abduct either eye, and the right was habitually crossed inwards. There was no positive paralysis of any of the other ocular muscles, but they all acted feebly, and he had the same sleepy look observed in other cases, from not being able to keep his upper eyelids properly elevated. He could, however, by effort, lift his eyelids. His left internal rectus seemed weaker than the rest. The optic

discs were both very pale, and the artery and vein much reduced in size. The atrophy was more advanced in the right. He still enjoyed good health, and, in spite of being almost blind, still acted as gardener and groom. I could not make out much as regards other evidences of disease of the nervous system. He was liable to attacks of severe pain in his forehead, sometimes lasting a week or more. For seven or eight years he had been liable, he said, to cramp in his legs at night. His bowels were constipated, but not extremely so. At this date his pupils did not act in the least—the left being of medium size, and the right rather larger. He had no habitual headache. I now prescribed mercury and pushed it to ptyalism, but with no definite benefit.

During the spring of 1874 I saw him repeatedly in the country, and about March the following note was made:—Pupils motionless. External recti paralysed, and all the others imperfect. The superior oblique appear to be also weak, but as he can still use the inferior recti a good deal it is difficult to test them. His bowels have been very costive, and he complains much of tightness round the abdomen as if a strap were round him, and also of numbness in the skin of abdomen and face. There is also slight numbness in hands and a little in feet. Up to this date the man had been able to get about, and used to drive himself in a pony carriage a distance of seven or eight miles to see me. Not long afterwards, however, an aggravation of symptoms took place and he became confined to his house. After this I did not see him again. I was informed that he had a “sort of choking fit” and soon after became absolutely blind. His extremities next failed him, and he had to keep his bed. He now suffered from dreadful pain in his head, and was frequently “out of his mind,” and liable to use most violent language. He could speak well and swallow easily, and used to eat largely. His lower extremities became quite useless and were usually “icy cold.” In this condition he died in May, 1876, about seven years after the commencement of his symptoms.*

* The following particulars were kindly obtained for me by Dr. Sloman, of Farnham. “About Christmas, 1874, loss of power in his arms was

Two very remarkable coincidences occurred in this case and by their aid I am enabled to complete its narrative. They are so peculiar that I must ask the Society's excuse if I briefly mention them. I had been very desirous to know whether this man had suffered from syphilis; as stated, he stoutly denied it, and he appeared to be straightforward. I got him to bring me two of his children and neither of them showed anything in the least suspicious. So matters stood when in April, 1876, Dr. Horace Jeaffreson of Wandsworth wrote to me that he had under his care a maid-servant who was the daughter of a man in whom I had taken much interest and that the girl was now suffering from inflamed eyes. He sent to me Robert S—'s eldest child, a girl of twenty, with *notched teeth and a most characteristic condition of syphilitic keratitis*.

This girl, who became my patient at Moorfields, informed me from time to time of her father's condition. I was very anxious to get Dr. Hughlings Jackson to see him, and for a year or more we had it in contemplation to go some day to Farnham and ascertain the present state of his symptoms. One Saturday afternoon in the spring of 1876 we accomplished our intention, and having found the man's cottage ascertained that he had died the day before. After much persuasion we succeeded in getting permission for a post mortem, and brought his brain home with us.

Dr. Gowers was kind enough to make for me a detailed microscopic examination of the cerebral nerves and their nuclei, and I append his report. It will be seen that degenerative changes precisely similar to those of progressive

noticed; he would drop a cup or anything he was trying to drink from. In January, 1875, his legs became weak, and he took to bed at the end of January or beginning of February; he was constantly in bed after that. During this time his arms and legs would from time to time "fly up" (as his wife expresses it), and as suddenly drop again. His lower limbs were constantly cold. He had no constant paralysis of the sphincters, but very occasionally urine was passed unconsciously. The bowels never acted at all without medicine. He could swallow perfectly, and also speak and taste. He had no paralysis or loss of sensation in muscles of face. He used to suffer much from headache."

muscular atrophy were found. These changes implicated the optic nerves and the third, fourth, and sixth. The fifth were slightly affected, but all the others were healthy. We had not been able to obtain any part of the spinal cord. From the nuclei of all the nerves mentioned the cells had disappeared.

Examination of the brain by W. R. Gowers, M.D.

The brain, when received, was somewhat softened from commencing decomposition, and the following facts are all that could be ascertained with certainty. The cranial nerves were examined microscopically, in the fresh state; the pons and medulla after hardening.

Nothing abnormal was observed in the convolutions. The corpora striata were apparently normal, the only exception being the presence of a hyperæmic patch in the left lenticular nucleus. The optic thalami were of normal size and consistence, except that the posterior tubercles were perhaps a little smaller than normal, and a little softer. Microscopical examination did not show any special change. The corpora quadrigemina were of nearly normal size; the posterior only being distinctly smaller than natural, but on microscopical examination they appeared healthy. The pons and medulla were of the usual size, and the only abnormality was an unusually deep central sulcus of the floor of the fourth ventricle, but this was found, on further examination, to be independent of any change in the structure of the pons.

Cranial nerves and their nuclei.—The *olfactory nerve* appeared normal. The *optic nerves*, and *chiasma* were uniformly grey, but of fair consistence. The optic bracts were also grey, but in places presented a white almost nacreous striation. Microscopical examination of the nerves and tracts showed many fat globules and degenerating fibres, but also a large number of healthy fibres. *Third nerves.*—Smaller than natural, grey and translucent. Very few healthy fibres could be seen in them; some fibres were seen undergoing degeneration, and there was a large number of

connective tissue nuclei. In the crura cerebri the passage of the tracts of fibres of origin was indicated by lines of connective tissue fibres in which scarcely any nerve fibres could be seen. Their nuclei beneath the nates presented striking changes. Almost all the large multipolar nerve cells had disappeared, two or three only were to be seen in each section. A few cells were seen of some size, but without processes. Others appeared represented by minute angular cells not larger than connective tissue nuclei. The latter were very abundant throughout the tract.

Fourth nerves.—No trace of these could be seen. They had probably become reduced to fine connective tissue threads, indistinguishable from the fibres of the pia mater. Their nuclei beneath the testis presented a similar degeneration to those of the third nerves in the anterior part of the same tract.

Fifth nerves.—The upper fibres of the large root appeared healthy, but the lower fibres had a grey appearance and presented granular degeneration and segmentation on microscopical examination. Within the pons there was little recognisable alteration in the fibres of the nerve, and the nucleus was for the most part normal. Here and there were patches of disintegration. The nucleus of the motor root of the fifth was in all respects normal.

Sixth nerves.—These were reduced to fine grey threads in which, under the microscope, scarcely a single nerve-fibre could be seen. They were made up of nucleated connective tissue, with here and there a row of granules to indicate the position which had been occupied by a nerve fibre. Within the pons lines of connective tissue alone indicated the course of their tracts of origin to the nuclei beneath the eminentiæ. These, the so-called “conjoined nuclei” presented general degeneration; most of the large nerve cells had disappeared and only granules, nuclei, and small angular cells remained.

Facial nerves.—Perfectly normal, both in their trunks and roots of origin within the pons.

Auditory nerves and nuclei normal.

Glosso-pharyngeal nerves and nuclei normal,

Pneumogastric nucleus normal.

Hypoglossal nucleus normal.

Throughout the medulla, pons, and corpora quadrigemina the perivascular erosions, so frequently met with, were very large and numerous. In the lower part of the floor of the fourth ventricle there were some areas of disintegration, in the grey substance just beneath the lining membrane; and the surface, partly from this cause, was more than usually irregular. One such area appeared to have been caused by a small hæmorrhage.

Remarks.—The degeneration of the nerve roots, and disappearance of the nerve cells from their nuclei of origin, are precisely similar to the changes seen in other parts in progressive muscular atrophy. There was no indication of pressure upon the nerves, nor of any acute change in their nuclei, nor in any centre. The disintegration and connective tissue changes are constantly met with in the grey matter of the cord in progressive muscular atrophy.

CASE 4.—In the following case no history of syphilis was acknowledged. It was, however, very closely similar to the others which were syphilitic; and when we remember what occurred in the preceding case in this matter, we cannot trust much to negative statements.

Abstract.—Almost complete paralysis (not equal) of all the ocular muscles, with the exception of the inferior rectus on right, and external and inferior on the left. Slow failure of sight in both eyes, but not symmetrical. Atrophy of discs with but little diminution of central vessels. A feeling of tightness across forehead and great irritability of temper. Patient a man of middle age in good health. Insanity threatened.

William M—, a florid healthy-looking man, æt. 45, came under my care in January, 1875. He considered that he ailed nothing whatever excepting his loss of sight. His pupils were of medium size and quite motionless. With his left eye he could see a hand, but could not count fingers, whilst with the right he could just spell out No. 20. His

manner was slow and dull; he answered me in monosyllables, and his wife said that at home he would often sit for hours and not speak. He slept much. He complained occasionally of dizziness, and said that he frequently had "a sort of tight pain" across his forehead. He had no pain in his limbs or joints. The account which he gave of his first failure of sight was, that it occurred when he felt quite well. He began to find his eyes dull, and they felt as if sand were in them. He had no sickness and could walk well. The left eye failed first. He attended at Guy's Hospital in 1873, and took phosphorus and nux vomica. A note on his Guy's paper, stated that "the optic discs were white and atrophic, blood supply to retina good." In June, 1874, he had an eruption on his legs, which he thought was due to the medicine. He describes it as purpuric.

When he came to me his expression was somewhat sleepy, from his not opening his eyes well, but there was no positive ptosis. The left globe oscillated somewhat from side to side. Many of his ocular muscles were paralysed. Thus, on the right side, the only muscle which enjoyed any material power was the inferior rectus. The others, although not absolutely paralysed, were almost so. On the left side the external rectus acted well, and the inferior fairly, but all the others only very slightly. When told to look upwards he simply converged his eyes. None of the muscles were absolutely paralysed, excepting, perhaps, the right external rectus.

The pupils dilated well with atropine. The optic discs were both of them pale, the left being much whiter than the other. The branches of the artery were but little diminished.

M. was by occupation a dock labourer, but he said that he often did not work more than two days a week, as he was lame in consequence of a compound fracture of one leg. He had been well fed and had a good appetite. He had always been temperate in stimulants, and had never smoked. His smell, taste, and hearing were perfect. He denied having ever had syphilis, or that he had experienced any sexual

failure, but in these matters he was not communicative. He had married, for the first time, eight years before I saw him. His wife was older than himself.

In March of same year he was in much the same state. His club doctor had certified that he was insane; but his wife denied it, and although irritable, he had always to me appeared rational.

CASE 5.—The case of F. W. M—¹ is a very characteristic one. He was an engine-fitter, æt. 39, in apparently good health, and free from symptoms of spinal disease. He had suffered from complete syphilis twenty-two years before the eye symptoms began. The latter, although ultimately symmetrical, were not so at first. In the beginning all the recti on the left side failed, the pupil became dilated and fixed, and accommodation was lost. Amaurosis on the same side followed, and two years later similar symptoms were developed on the right side, and in the same order. Under a prolonged but perhaps too mild anti-syphilitic treatment his disease advanced very slowly. Six years after his admission, and nine from the commencement of his symptoms, he was quite blind, and his eyes almost fixed. He still possessed, however, slight power over certain of the recti. Excepting some occasional shooting pains in the head, and a degree of numbness of the forehead, he had not experienced any other nerve-symptoms, and was when last seen in good general health.

CASE 6.—The case of John H—² a labourer in wine-vaults, æt. 34 is of special interest, because symptoms of locomotor ataxy preceded those of ophthalmoplegia. It was not certain that he had ever had syphilis, and excess in venery, alcohol, and tobacco were amongst the possible causes. Obstinate constipation, retention of urine, impotence, and weakness of the lower extremities, were his earliest symptoms. Then followed symmetrical amaurosis and paralysis of the recti muscles on both sides. The pupils

¹ Detailed notes of this case extending over nearly six years were appended to the paper.

² Notes of this case also accompanied the paper.

had become dilated and motionless before the ocular muscles failed. He ultimately became almost blind.

CASE 7.—The next case which I shall mention is the last which has come under my notice, a very definite example, and one in which I had the pleasure of producing the patient for the examination of the Society on a former occasion. The patient is a butler in a country house. He is thirty-four years of age, and about ten years ago he went through a sharp attack of syphilis. He has now been for three years or more the subject of ophthalmoplegia externa in combination with some symptoms of ataxia. I have had him under treatment for about a year, and under specifics in full doses he has greatly improved, but the movements of his eyeballs are still in all directions much restricted. None of the muscles are absolutely paralysed, but all are very feeble. It is important to note that his pupils act fairly, and that his accommodation is perfect. His lower extremities were, at one time, very weak, and liable to much aching pain. He walked badly, and used occasionally to fall. These symptoms have now passed off, and he walks well, and appears to be in excellent health. This patient had taken the iodide before I saw him, and there is every reason to believe that it has been the means of saving his life, for, at one time, the symptoms were rapidly progressive, and he was confined to bed.

CASE 8.—Mr. Ilott, of the Bow Workhouse Infirmary, sent me in 1878 an interesting example of this malady in connection with a syphilitic history, and with modified symptoms of locomotor ataxy. The patient was an actor, æt. 30, of much intelligence, who had already been under hospital treatment, and who told me spontaneously that he had suffered from syphilitic disease of the brain. He had had syphilis eight years before, and at the time that his nerve symptoms began he was under Mr. World's treatment for a tertiary ulcer on the leg. The onset of his symptoms was, according to his account, sudden. He one day felt a "click" in his head and fell down in the street. He got up,

walked to an omnibus, and went home, but ever afterwards one eyelid drooped; a few weeks later the other eyelid drooped, and for a time both eyes were closed. Under treatment by iodide of potassium, extending over a year, he recovered, and when he came to me he considered himself well, and was not taking medicine. I found his pupils motionless, and of medium size, but accommodation almost perfect in both. His lids drooped to a moderate degree, and, excepting the internal rectus on each side, almost all his ocular muscles were more or less paralysed. The conditions were almost symmetrical. He could see well, and suffered no pain. His gait was decidedly ataxic, and he complained that his legs were weak. The sexual function was almost wholly in abeyance.

The subject of the above case was presented before this Society last year, and a week afterwards Dr. Gowers was kind enough to send to me for examination the subject of the following as an example of similar disease.

CASE 9.—A servant girl named B—, æt. 27, had been under Dr. Gowers' treatment for several years. Four or five years ago, after much headache, both eyelids began to droop, and subsequently the ocular muscles on both sides became involved. Her pupils continued to act well, and she retained sight and accommodation. At the time of my examination almost all the muscles were defective, and some almost completely paralysed. The inferior recti and the inner rectus on right side had most nearly escaped. There were some indications of ataxy. Her lower limbs were weak, and she walked badly, complaining that she could not go up stairs easily, and that sometimes her legs seemed to give way. Once she had tripped and fallen. I did not ask direct questions as to syphilis, and there was no other clue to it.

I am indebted to my colleague, Mr. Waren Tay, for the notes of two cases in which young children were the subjects of symmetrical paralysis of the muscles of the eyeball. In neither of them was there any proof of inherited syphilis, but in one the paralysis passed away under treatment by specifics in a manner which was very suggestive.

CASE 10.—A female infant, *æt.* 9 months, under care in 1876, at the London Hospital, had symmetrical ptosis and almost complete paralysis of all the ocular muscles. The lids drooped so as almost to cover the eyes. It was very difficult in so young a child to test each muscle, but so far as could be ascertained the paralysis was almost absolute, with the exception that the left eye moved a little both outwards and inwards. The condition had existed for fourteen days. The child was pale and fretful, but showed no signs of syphilis. There was an elder child, *æt.* 3, also healthy, but both of them had suffered from “thrush.” Iodide of potassium and mercury were ordered, and at the end of four months all trace of the paralysis had disappeared, and the child was cheerful and in good health. Five months after treatment was laid aside the child was seen again, and continued quite well. Two months after this, however, it was taken ill again, wasted, had convulsions, and died, but no information of this illness was given to Mr. Tay until some time after the death.

CASE 11.—The subject of Mr. Tay’s second case was a girl, *æt.* 3, named A—, under care in March, 1878. Both eyelids drooped considerably, and several of the recti on both sides were paralysed. With the left eye she could look outwards but not in other directions, and with the right inwards and outwards fairly, but not in other directions. The pupils were of medium size, and sluggish. The symptoms had been coming on for six week, and there was a history of a slight fall. The child was fretful, but there were no indications of syphilis. A boy two years older than the patient was quite healthy. Iodide was ordered. Death occurred on April 19th, 1878, but no details of the latest symptoms are forthcoming, nor was a post-mortem obtained.

CASE 12.—I have met with one instance of this malady which was unquestionably in connection with inherited syphilis.

Abraham F— is at present unable to use any of the recti

muscles excepting to a slight degree, and his eyes are almost fixed in a divergent position. He is quite blind, and both discs are in an advanced state of atrophy. He has been at different times during the last few years under many of the ophthalmic surgeons of London, and also under Dr. Hughlings Jackson, by whom he was sent to me. He is florid and healthy-looking, but with a suspicious forehead, and quite characteristic teeth.

At the age of 13 his sight began to fail, and squint was noticed, and from this time his symptoms steadily advanced. For two years and a half he has been, as he is now, totally blind, with fixed eyeballs, and slightly drooping lids. There has been no evidence of increasing disease of the centres, and he still walks well, and has perfect use of all his faculties. He sleeps and eats well, and goes regularly to a blind school, where he reads by touch. He has now no headache, but in the earlier stages he had much headache and occasional "shiverings." His age is now 16.

His right eye is usually on a lower level than the left. He can only by great difficulty lift his lids so as to expose the whole of the cornea, and when at rest the corneæ are half covered, and he looks as if just going to sleep. Both eyes diverge. Both inferior recti are wholly paralysed, and both superior almost wholly so, the left being rather better than the right. The internal rectus of the right side is quite paralysed, and that of the left nearly so. The external rectus of the right is very feeble, but that of the left still acts moderately, being much the most active muscle on either side. All the obliques are paralysed.

CASE 13.—*Symmetrical and almost complete ophthalmoplegia externa without implication of the iris or ciliary muscle, but with defect of all muscles supplied by the fifth, and, in slight degree, of those by the seventh. No history of syphilis.*

A young man named George S— came to Moorfields in October, 1877, and was admitted under Mr. Waren Tay's care. He showed no signs of inherited syphilis. He was only 19, and entirely denied any history of acquired taint.

He was from Devonshire and of healthy family. In addition to investigations by Mr. Tay and myself, his symptoms were carefully studied by Dr. Hughlings Jackson, Dr. Barlow, and others. On both sides his eyelids drooped so as to almost cover the eye, and all the muscles of the eyeballs were exceedingly feeble. His pupils, however, acted well, and accommodation was perfect (entire absence of ophthalmoplegia interna, and probable integrity of the lenticular ganglion). The ocular muscles were somewhat more completely paralysed on the right side than the left, and the lid drooped more, but on both sides the degree of weakness was almost complete, and involved all the muscles. In addition to the muscles of the eye, all those supplied by the fifth nerve were on both sides defective, but their paralysis was not nearly complete, and on both the muscles supplied by the facial were also slightly weak. There was no defect of sensation, nor any symptoms referable to the spinal cord. The vision and other special senses were perfect, and the man appeared to be in good general health. There had never been much headache.

The symptoms above mentioned had been present six months, and were supposed to be consequent on a trivial blow from a piece of wood on the left temple. There had been an interval of three weeks between the blow and the first drooping of the eyelids, and the blow itself was so slight that it may be reasonably doubted whether it had any connection with the paralysis.

Through the kindness of Mr. Fernie, of Barnstaple, under whose care the man is, I am enabled to state that at the present date, fifteen months since he was under care at Moorfields, the symptoms remain without advance, but with some improvement. Treatment by iodide of potassium has been pursued, but not regularly, nor in large doses.

I may suitably place in juxtaposition with this case one in which, as in it, the fifth-nerve muscles were involved in association with those of the eyeball. It differs, however, definitely from the preceding one in that there can be no doubt that the patient is the subject of syphilitic taint. It is a very

remarkable case, but I must state its facts as briefly as possible.

CASE 14.—Madame de T— æt. about 30 years, and in good bodily health. She has notched teeth, and has suffered a severe attack of interstitial keratitis. A sister, like herself, is the subject of inherited syphilis. Before, however, we confidently attribute her nerve-symptoms to inherited taint, I am bound to state that her husband believes that she has had acquired disease as well. About this no certainty is attainable. She is at present the subject of paralysis of all the ocular muscles on the left side, with only slight weakness of these on the right, but with double paralysis of the fifth, both motor and sensory, and some defect of both facials. She was formerly hemiplegic in her right limbs, but this has passed off. The account of the development of her symptoms is obscure, but the paralysis of the cranial nerves preceded the “fit.” She has already derived much benefit from treatment by large doses of iodide of potassium. The non-symmetry of the ophthalmoplegia, and the occurrence of hemiplegia, separate this case somewhat from the group under our consideration.

I have seen double paralysis of the ocular muscles once—and only once—in association with a history of apoplexy, and the case, although probably not very closely cognate to the others, must be briefly mentioned.

CASE 15.—Abraham B—, a healthy-looking old man, æt. 72, came under care in 1874, having, on the right side, paralysis of all the muscles of the eyeball, except the internal rectus. The condition was symmetrical, but on the left side the degree of paralysis was not so great. The eyeballs were converged, so as to be almost buried in the inner canthi. Accommodation was lost, but vision almost perfect. He was partially hemiplegic in the left limbs, and the muscles supplied by the right facial were weak. The history was, that the symptoms had followed a fit which had occurred suddenly three or four years ago. There was no history of syphilis, but its possibility was not denied. His vision failed

during the period that he was under observation, but in other respects his symptoms were stationary. It is difficult on any hypothesis of hæmorrhagic disorganisation to explain such a group of symptoms, and it is, after all, quite possible that the apoplexy was an accidental concomitant, and that the ophthalmoplegia was of the same nature as in the other cases.

P.S.—Since my paper was sent in I have found in my note-books two cases of this affection which I have omitted to mention.

CASE 16.—The subject of the first is a married lady whom I attended with Dr. Hughlings Jackson some years ago. She had double ptosis, with incomplete paralysis of most of the ocular muscles, fixed pupils, and loss of accommodation. She had been liable, previously, to most violent attacks of vomiting (“abdominal crises”). She improved very much under specifics. She became subsequently hemiplegic. She is still living (ten years after the first symptoms), but is confined to her room, and many of her ocular muscles still paralysed. There is much reason to suspect syphilis.

CASE 17.—The subject of the second is a sailor, who had syphilis six years ago, and in whom both-sided paralysis of the eye-muscles with ptosis set in rather suddenly after two months’ premonitory symptoms in the form of failing accommodation.

I have detailed notes of both these cases, but must not further trespass on the patience of the Society. It will be seen that these add two more to the list of those in whom the disease appears to be due to syphilis.

Comments on the Series.

The facts are too few to permit of statistical analysis, but a brief summary may, perhaps, be useful. Of the fifteen cases four only occurred in females, and two of these were young children. One of the males was a boy the subject of inherited taint, and the remaining ten were all adults. In eight of the fifteen it seemed certain that syphilis was the

cause, in six acquired, and in two inherited. Of the remaining seven it may be said that a reasonable suspicion of syphilis might be entertained in several. Had it not been for an almost accidental revelation of the truth in the case of the man S— after prolonged fruitless investigation, I should certainly have asked the Society to believe that his was an example of the disease without any probability of syphilis. With such a fact in mind, one feels that it is almost impossible to make the negative even fairly probable. In Case 9 no direct question was asked, the patient being a single woman. In the two young children, although nothing could be proved, there were some suspicious facts, and one recovered under the iodide. The case in which evidence is most conspicuously absent is No. 13, but in this the patient is a young man, who may have denied the true history, or who may have either inherited a taint or had the acquired disease in some irregular way, of which he knew nothing. On the whole, the evidence which connects this affection with syphilis is exceedingly strong, and that which favours the belief that it can occur independently of it, must be held to be open to some doubt.

In five of the patients (all but one, known to be subjects of syphilis), the optic nerves were affected, and blindness, with white atrophy, resulted. In two cases the fifth nerves were symmetrically affected, and in two there was slight affection of the facials. The almost constant escape of the facials must be held to be a remarkable fact. In one case the palate was affected, and in one, the same patient, smell was lost. None of the patients were deaf and none had lost taste, and in only one was there any material anæsthesia of the skin of the face. In six cases the lower extremities were more or less weak, and liable to pain, the condition approaching more or less closely to locomotor ataxy. I much regret that, owing to imperfect knowledge on my part at the time of their occurrence, the details of facts in reference to this disease are often incomplete. There can, however, be no doubt that ophthalmoplegia externa is sometimes a part of the general malady known as progressive locomotor ataxy,

especially when that disease is due to syphilis. One of the patients became insane, and another was before death liable to attacks of violent mental excitement. Four of the fifteen are known to be dead, but of these a post mortem was obtained in only one. In several cases, owing either to the blindness or the youth of the patient, it was impossible to estimate the state of the accommodation, but in a certain number it was proved to be perfect, in a few it was absent, and in a few it was impaired. The pupil, never contracted, was almost always sluggish and of medium dilatation. In one well-marked case it acted fairly. In no single case was it very widely dilated. From these facts we may infer that the lenticular ganglion is often free from disease, and that the vaso-motor filaments, although often enfeebled, are not usually paralysed.

It is difficult to make any confident statements as to the progressive tendencies of the malady of which external ophthalmoplegia is a symptom. It is very definitely influenced for good by treatment, and in nearly every case specific measures were adopted. We may conjecture, however, from what happened in several, that in most instances it is an aggressive malady, and would end fatally if treatment were not resorted to. The effects of remedies in several of the cases were very remarkable, the patient being rescued from a very dangerous condition. At the time that most of the cases were under treatment my opinions, as regards the nature of the malady, were far less clear than at present, and hence a hesitancy in treatment, which was probably often prejudicial. The patients were benefited up to a certain point, but relapses occurred, and the remedy was not pushed sufficiently. It would seem that iodide of potassium is by far the best means of treatment, and that it ought to be given over very long periods, and in increasing doses without any limit as to precise quantity, excepting its effect on the symptoms. Although relapses are common, yet in several of the cases narrated a complete arrest appears to have occurred, no treatment having been resorted to for several years. In none, however, was the recovery complete.

ON
SYPHILIS AS AN IMITATOR.

AN ADDRESS DELIVERED BEFORE THE LANCASHIRE AND
CHESHIRE BRANCH OF THE BRITISH MEDICAL
ASSOCIATION.

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SYPHILIS AS AN IMITATOR.

MR. PRESIDENT AND GENTLEMEN,—An idea has much occupied my mind, during the last few years, which seems to considerably simplify our creed as regards the nature of the very various forms of disease which are caused by syphilis. It is, that we ought to recognise, in syphilitic forms of inflammation, imitations, with certain differences, of the other and more common types of diseased action which occur without any connection with specific taint. The fact, as a matter of clinical observation, must have been obvious to all for long; and the names which the various types of syphilitic eruption have received recognise it. As a rule, we are content to apply the adjective “syphilitic” to some substantive which designates a well-known form of non-specific skin-disease, “syphilitic psoriasis”, “syphilitic roseola”, and the like.

This recognition of the doctrine which I purpose to propound to-day, in some detail, is so general and so old, that I might, perhaps, almost need to apologise for making it the subject of a special discourse, were it not that I hope to be able to show that it has ramifications which have as yet scarcely received the attention they deserve. To my own mind, I may acknowledge that this general law of imitation or simulation has been of the greatest possible service; and that it has helped me to a clear view of much that was previously obscure. There is no difficulty in explaining why syphilis, in its action upon the solids and fluids of the human body, should be an imitator of old types, and not an originator of new ones, if we admit, as probable, that almost all possible forms of morbid process existed already independently of it. The introduction of the syphilitic virus into the system simply gives a tendency to derangement of cell-nutrition, resulting in local inflammation. All such derangements must be regulated in their development by anatomical conditions and physiological laws. Certain structures exist which will be attacked, and their number and variety is not unlimited. There are in

the skin, for example, certain glands, papillæ, vascular areas, and special appendages; and any of these may be attacked by disease; and the possible variety as to its anatomical location, although great, is still restricted. Syphilis cannot devise for itself any new set of organs, nor can it lay down any new lines along which morbid action, once originated, may spread. It must work in the old grooves, and travel by the existing paths.

A writer, much read in my youth, and now much neglected, remarks, respecting the so-called author of all evil, exactly what I wish to assert of syphilis. "He can mar, but he cannot make; for he is not antagonist deity." In thus denying the original ability of Satan, Mr. Tupper expresses in the language of theological metaphor that which is capable of easy explanation by natural laws. Our vices and our faults are only the results of unbalanced or insane instincts. There is not one of them but has a natural basis in something of itself perfectly laudable. Avarice is only exaggerated prudence; lust springs from virility; envy, covetousness, and the tendency to steal, find their roots in the perception of what is desirable, and the too energetic zest of possession. Given a knowledge of virtue, and we can interpret vice. Now, it appears to be almost exactly so with syphilis amongst diseases. Given a knowledge of other types, and we can foresee on what plans it will work. The number of its simulations is wonderfully great. I will enumerate briefly some of the most remarkable of them.

In connection with what some name the darts diathesis—an unknown peculiarity of health, or of skin, which gives proclivity to skin-affections—we have several very definite and distinct forms of eruption: first, *common psoriasis*, an eruption consisting of increase of epidermal elements in patches, with vascularity and swelling of the derma beneath; next, *lichen-psoriasis*, or *lichen ruber*, consisting of grouped lichen spots, which may merge together and become smooth and polished, but are rarely very scaly; thirdly, *pemphigus*, the eruption consisting of bullæ, which contain clear serum. To these might be added a number of minor modifications less generally known, and which it would be tedious to describe. All these agree in that they occur to healthy persons, prefer young adults, never occur to infants, never get well of themselves, relapse after apparent cure, and are always influenced for good by arsenic and tar. It is clear that they are modifications of the same malady; and although in external appearance very different, yet their essential resemblances (just enumerated) far exceed their apparent differences. We may suspect that their differences depend upon peculiarities in the indi-

viduals who become their subjects, and that their cause is in all the same. Now, it is of extreme interest to note that, in syphilitic secondary rashes, we have all these reproduced, thus proving that one and the same cause may in different persons produce very different results. Syphilitic psoriasis is well known; syphilitic rupia is the representative of non-specific pemphigus; and those familiar with lichen ruber will not unfrequently find in syphilitic eruptions an exact imitation of it. But the varieties of the syphilitic secondary rash are by no means exhausted by its reproduction of the common forms of the darts of the malady; for we have also imitations of the exanthems, and of some much more rare types of eruption. Syphilitic roseola is, so far as the anatomy of the lesion is concerned, measles; certain forms of diffuse erythematous lichen closely resemble scarlatina, and there is a pustular rash exactly like variola. This last imitation by syphilis is so remarkable, and may lead to such serious errors in diagnosis, that I must advert to it in more detail. It has been carefully described by Dr. Liveing.

In the form of syphilitic eruption which looks like small-pox, the rash is scattered symmetrically over face, limbs, and trunk; it may be discrete or confluent, according to its abundance; the pimples are hard and shotty at first; they have depressed centres, they form adherent scabs, and they leave scars. The eruption differs, of course, from true variola in the slowness of its evolution, in its long persistence, and its very slow decline; and it is only in its early stages that mistakes in diagnosis are likely to occur. Even in these, the comparatively small amount of febrile disturbance, and the absence of backache, will usually excite suspicions which may probably be converted into certainties by examination of the patients' genitals, and the discovery of a chancre. One of the most remarkable examples of this eruption came under my notice about twelve years ago. A young gentleman called on me with a conspicuous papular eruption on his face and other parts. "I have just had small-pox", he said; "and Mr. — says that I am cured; but the spots don't go away." He added that Mr. —, a gentleman of large experience, had kept him in bed a fortnight, and had since sent him into the country for a fortnight, and now said that he might return to his desk at a bank; "but", he continued, "the other clerks won't sit near me, and declare that I have small-pox still". He had a chancre, and the eruption was syphilitic. I have seen several cases which had been treated in the small-pox hospital for eruptions which were undoubtedly syphilitic. But I must not mention the mistakes of others, unless I am prepared to be candid about my own. I had many a time

in clinical lecture mentioned the preceding facts, and enlarged upon the importance of distinguishing between the syphilitic simular of small-pox and the reality, when my own turn came. One day in the summer of 1877, I was hastily summoned to see a gentleman at his own house, who had just been landed from a sea-voyage, during the whole of which he had been very ill. He had been carried from the vessel to his house, and put to bed; and I found him covered from head to foot with crusts exactly like those of variola in the third stage. Some had fallen, and, where this had happened, deep scars were left. The eruption had begun to come out on the day that he went on board, and he had been feeling ill a few days before. The stages had been unusually long, but still had not exceeded possible limits. I questioned him as to syphilis, and examined his penis and his throat, but without finding any reason to doubt his denial. In a word, after a careful and sceptical investigation, I thought that the eruption was variola. The sequel proved that it was syphilis; the scabs took months to fall; and, just when he was recovering from the eruption, he had iritis, which I could not doubt was specific. At this stage, three or four months after I had seen him at home in bed, he came to Moorfields Hospital. His face was pitted all over, and I had much difficulty in convincing those who then saw him that he had not really had small-pox. I could not quote an instance more conclusive in support of the assertion that one of the forms of syphilitic eruption is exactly like small-pox in all its stages, and in its resulting scars. Slow progress is the one difference between the two exanthems. The similarity is produced, no doubt, by the fact that syphilis in these cases attacks precisely the same anatomical structures as those in which the variolous pustule is developed. It is scarcely needful to remark that this form of eruption always occurs in the secondary stage.

Syphilitic *roseola* is well known. It is a very common form of rash, usually occurring in the very beginning of the secondary phenomena. We have a mottled congestion of the skin, in ill-defined crescentic patches, exactly like those of measles. As in measles, it is almost wholly an erythema—that is, a dilatation of blood-vessels in patches; and, if the skin be stretched, the vessels may be emptied, and the rash vanishes. Syphilitic roseola is less bright in tint and less conspicuous than measles, but in all other respects the eruptions are alike; and no doubt they are produced under the same laws as regards the local vascularisation of the parts. In other words, there exists, no doubt, in the skin a certain arrangement of areas of capillary vessels which, under irritation, permits of temporary paralytic dilatation, with the result of

patches of congestion peculiar as to size and shape. The poisons of syphilis and of measles alike possess the power (in common with certain other causes—copaiba, for instance) of so irritating the vaso-motor nerves as to cause the dilatation of these pre-existing tufts.

Syphilitic *pemphigus*, a bullous eruption, in the first instance exactly like common pemphigus, rapidly changes into what is more usually styled rupia. Under the latter denomination, it is attended by heaped scabs, and by ulcerations at the base, and differs much from any non-syphilitic eruption. It is, however, of the initial lesion that I chiefly speak; for it is to be admitted that all syphilitic imitations are most close in their beginnings, and that they all develop peculiarities after lasting for a time, which much facilitate their differentiation. These peculiarities are mainly a tendency to persist long, a tendency to suppurate and to ulcerate, and, third, a tendency to be serpiginous—that is, to infect the adjacent tissues, and thus to creep over considerable areas.* This latter feature belongs especially to diseases in the tertiary stage, and is seldom seen in the secondary.

The eruption described by Wilson under the name of *lichen planus*, and by Hebra under that of *lichen ruber*, and for which I much prefer that of *lichen-psoriasis*, is a peculiar and interesting one. An adult in good health suddenly becomes the subject of a symmetrical eruption, which consists of dusky lichen spots, arranged in groups which are often linear, and never round, and which tend to coalesce and form patches, which become smooth and polished. It is not syphilitic; it is curable by arsenic and tar, just as psoriasis is, and, like it, it is liable to relapse. Clearly the two are very closely related, differing chiefly, perhaps solely, in the local anatomical lesions. Now, although, as stated, the original type of this malady is certainly non-specific, and requires arsenic and not mercury for its cure; yet a syphilitic imitation of it, in which the resemblance is most close, is very common. I have myself many more opportunities for studying syphilitic lichen ruber than the original malady. I show here an excellent portrait of it, taken from Mr. Wilson's *Atlas*, and to which is affixed the name lichen syphiliticus corymbosus. It was published before lichen ruber was recognised, and is a good proof of the originality and accuracy of Mr. Wilson's observation.

It would be tedious to enumerate all the minor varieties of the eruption which occurs as part of the secondary stage of syphilis, and to

* This curious fact supports the belief that in the secondary stage the poison is free in the blood, and that in the tertiary it is the solid tissues which are altered. Thus in the secondary all parts are equally supplied by the poisoned blood, and individual patches do not creep.

attempt to find their parallels. It may well suffice to say that there are several less frequent forms, both in relation with the dartsy diathesis and the syphilitic state, and that their laws of similarity appear to be just the same as in the definite examples which I have quoted. I will pass on to consider certain affections which occur in the later periods.

A disturbed state of nutrition of the skin in the palms and soles, resulting in breaking up of patches of epidermis, the formation of fissures, etc., is a common condition some years after syphilis, and is usually called *psoriasis palmaris*. It has nothing to do with true psoriasis. So frequent is it in association with a taint of syphilis, that I believe many think that it is in itself conclusive evidence of it. Such, I am convinced, is a great mistake. So-called psoriasis palmaris is often a senile change, and is much helped by local irritation, the use of tools, etc., and it may occur in connection with nerve-disturbance, as for instance, in locomotor ataxy; not, perhaps, in more than half its examples is it of syphilitic origin. The fact that there exists such a widely spread creed as to its being always specific, is conclusive in support of my proposition, that its syphilitic imitation is exceedingly close to the original. Only by very minor features, appreciable only to the most experienced, can the differential diagnosis be made. In many instances, it cannot be made at all, and nothing but the patient's history can help us in the conjecture as to its real cause. Often, no doubt, syphilis predisposes, and other influences help.

Lupus is one of the maladies which syphilis often simulates. Common lupus has, I must hold, no connection whatever with syphilitic taint, either inherited or acquired, but is certainly an appanage of the scrofulous dyscrasia. Overwhelming evidence on this point, and in confutation of those who think it always due to syphilis, might easily be given, but would require more time than is at my disposal, and I must ask to be allowed to assume the fact as proved. Now, the lupus process consists essentially in the growth of cell-structures (possibly adenoid) in the true skin, which destroy the tissues, and, when they retrograde, leave a scar. It is a necessary part of lupus that it should spread serpigiously. Lupus presents us with many varieties; sometimes it ulcerates easily, and sometimes scarcely at all; often it occurs in a single patch, and now and then it is multiple. Now, syphilis mimics it in all its varieties. There is a rare form of eruption usually seen at the end of the secondary stage, in which multiple patches appear in various places on both sides, but rarely symmetrically. These patches look, at first sight, like psoriasis, but, on closer inspection, it

is observed that they spread at their edges, and that they leave scars. They are in reality lupus, and have their prototype in the form of lupus illustrated in this portrait, in which it occurred in a scrofulous child as a multiple eruption. The common horse-shoe ulcer of syphilis is of the lupus type, being attended by new growth, spreading serpiginously and leaving a scar. It is usually tertiary. There is yet another form in which the resemblance is to ulcerating lupus. The nose is the part usually attacked, and it destroys a large part of the organ very rapidly. In bygone years, I have known it diagnosed as escharotic lupus, a term which denotes its rapidity of spreading. In it, the ulceration is at times phagedænic. I show some photographs illustrating this malady. It occurs both in the acquired and inherited disease, but is most common in the latter. It is most important to distinguish this imitation from true lupus, for it may be cured at once by the proper measures, and if neglected, rapidly effects great destruction. It may be distinguished from common lupus, alike by its rapidity, and by the fact that, after it is cured, the scar soon assumes a perfectly healthy state, and that there is no tendency to relapse.

I have seen some rare forms of serpiginous non-ulcerating syphilitic lupus, in which, even with the aid of the history, I have never been able to feel sure of the diagnosis until helped by the results of treatment. But such close counterfeits as these are rare. Let me insist that, in all these varieties of simulated lupus, a correct diagnosis is most important, for there is the greatest possible difference in the results of treatment.

Before leaving this part of my subject, let me ask attention to the fact that syphilis can simulate such peculiar conditions as those presented by Jacobs' ulcer or *rodent cancer*. This ulcer occurs almost solely in the upper parts of the face, and is known by its hard sinuous edge, which is usually free from warty growths, and presents a smooth, even roll of induration. It may heal in the centre, or may be attended by a tolerably clean excavated ulcer. The simulation of these conditions by syphilis is rare, but sometimes very close. I am tolerably well experienced in the diagnosis of rodent ulcer, having seen hundreds of cases; but I may admit that I have two or three times seen hard-edged syphilitic sores which so much resembled it, that, for a time, I was in doubt as to the diagnosis. Here is the portrait of one such, in which I was obliged to write to the surgeon who had known the antecedents of the patient before I could feel sure. The ulcer healed quickly under specific treatment. This simulation of rodent ulcer occurs, I think, only

on the parts on which rodent ulcer is common, and in patients past middle age. I have even seen a few cases in which the realities of rodent ulcer and of syphilis seemed to be mixed.

We will pass now from the skin to the *eye*, and use that as a bridge of connection, from which to enter upon a brief notice of syphilitic counterparts in connection with the nervous system. That syphilitic iritis is so like other forms of iritis that, excepting by its history and concomitants, it is, in nine cases out of ten, indistinguishable, is a point too well known to need insistence. Only in a small minority of cases do the tubercles, which, when present, are so valuable for diagnosis, show themselves. Interstitial keratitis, syphilitic in nineteen cases out of twenty, and almost always so when both-sided, may, in rare instances, find its type in the results of other causes; and the same is the fact respecting punctate keratitis, and some of the more peculiar forms of kerato-iritis. But the more noteworthy simulations occur in respect to choroiditis disseminata and retinitis pigmentosa. I show you, first, some drawings illustrating the non-syphilitic forms, and ask you to compare them with others of the latter class. In the two, the process is essentially similar, as closely so as we have just seen to be the case in the instance of lupus-disease of the skin. Patches of choroidal tissue are atrophied at the seat of deposit, which undergoes absorption and leaves areas of denudation. In both, the process is very liable to relapse and to spread. The syphilitic form is by far the more common, and this fact may often help the diagnosis. The imitation of retinitis pigmentosa by syphilis occurs almost solely in the inherited form of the latter disease, and the diagnosis may usually be made by observing that it is not symmetrical, but that one eye suffers much more than the other. When this malady is definitely non-symmetrical, syphilis should be suspected. Some interesting facts, illustrating the closeness of this simulation, have been published by Dr. Fitzgerald of Dublin. In retinitis pigmentosa, whether syphilitic or otherwise, we have an example of slow changes, which steadily advance during many years, disorganise the percipient structures, and are attended by widely-diffused pigment accumulations. It is rather of the nature of a progressive degeneration than of inflammation. The non-specific form is markedly hereditary, and is often in apparent connection with consanguineous marriages. That such a peculiar malady should find a close simulation as a consequence of inherited syphilis, is a very singular and instructive fact. The knowledge that such is the case helps, as I think, to interpret what occurs sometimes as regards the cranial contents. A certain proportion of the sub-

jects of inherited syphilis become slowly almost idiotic, and, without showing positive paralysis, show evidences of some progressive degenerative change in the grey matter of the brain. These patients sometimes find their way into lunatic asylums. In the absence as yet of definite information as to pathological anatomy, may we not plausibly suspect that the changes are allied to those which attend retinitis pigmentosa?

The epilepsy which is such a frequent occurrence as a result of syphilis, both acquired and inherited, probably depends in different cases upon somewhat different local lesions. Sometimes there are other special symptoms which disclose the real nature of the case, but often there is nothing but the patient's history to help to a diagnosis, and to reveal the fact that a cure may be expected from the iodide of potassium.

It will be evident that, in most of what I have said, I have been speaking, not of mere resemblances, but of imitations brought about on plan. But there are certain events in syphilis to which the term resemblance would be more appropriate. Such is the "syphilitic fit", or attack of apoplexy. This by no means unfrequent occurrence supplies often a very close resemblance to an ordinary senile "stroke". There is a short period of confusion, then insensibility, and then paralysis—it may be, hemiplegia. Sometimes several fits, at shorter or longer intervals, occur. These are to be distinguished, however, from what they resemble, in that they are not caused by similar local changes. There is not rupture of blood-vessel and hæmorrhage, but plugging of a vessel and arrest of circulation. They are more nearly parallel to the rare cases of apoplexy from embolism; but from these, again, they differ in that there is local disease of the vessel, which finally becomes thrombosed, not a sudden impaction of a floating plug; and hence certain minor differences in the development of the fit, which, however, I must not now stop to mention.

Under the term Locomotor Ataxy, we recognise a condition of liability to nervous disorder, one of the most conspicuous symptoms of which is a failure of the power of muscular co-ordination in the lower extremities. It is clear that this symptom is only one part of the malady, for the patient is liable to other and quite distinct forms of paresis. He may become blind, may have ptosis or ocular paralysis, and is liable to severe attacks of disturbance of the abdominal organs. The pathology of the malady is to a certain extent made out, and to some extent is yet obscure. It seems clear that low forms of chronic

inflammation, leading to permanent degenerative changes in the posterior columns of the cord, occur; and that these processes, once initiated, tend to spread in certain definite paths. It seems clear that the vasomotor system is often much involved. We do not, however, yet fully understand the bond of connection between the different groups of symptoms; nor do we know the special causes by which they are produced. It seems probable that a variety of causes may in turn or together take shares in causing this curious and very common malady. Now, one of the most remarkable facts in the history of syphilitic simulation is, that all the phenomena of locomotor ataxy may be, and often are, closely imitated. So close is this imitation, that it becomes one of the most difficult tasks in diagnosis with which I am acquainted to distinguish between them. Case after case comes under my observation with the suggestion that the patient is suffering from a syphilitic taint, and I find that all the symptoms range themselves under the head of ataxy. The small and sluggish pupils, the feeble bladder, torpid bowels, the amaurosis, the ptosis and failure of ocular muscles, the stabbing pains, the defective sensation in the feet and diminished coordinative power, the whole group of symptoms, not omitting the abdominal crises, all are there, and yet all are remediable to some extent, if not wholly, by specific treatment. Excepting in the facts as to history, and in the effect of specifics, there is little or nothing upon which to base a differential diagnosis. Whatever may be the parts attacked in non-specific locomotor ataxy, and whatever is the tendency as regards local spreading, it is clear that syphilitic disease may affect the same parts, may spread in the same directions, and may produce precisely similar results. If the question be put, Is there really any non-specific type-form of locomotor ataxy? is it not rather probable that the name is given solely to the results of serpiginous syphilitic changes in the nervous centres? we must, I feel sure, answer it definitely in the negative. It is probable—nay, it is certain—that the disease may begin independently of syphilis; and, notwithstanding the frequency of the latter malady, probably not more than half of those who become the subjects of ataxy have ever had it. It is certain, then, I repeat, that we have side by side two maladies almost exactly alike, one specific and the other not so. It may be asked further whether, in the cases in which ataxy happens in syphilitic persons, it is not yet probable that the ordinary causes are its real producers, and the syphilis merely an accidental concomitant. To this, again, a decided negative answer must be given as regards, at any rate, a considerable proportion

of cases. They are really syphilitic, and are curable only on that hypothesis. On this point, valuable evidence has been produced by M. Fournier of Paris, who has published several remarkable cures by the iodide of potassium, and who urges that, whenever the patient has had syphilis, this drug should be carefully pushed.

In support of the belief that there is such a thing as a slow serpiginous form of inflammation which spreads insidiously amongst the nuclei of special nerves and in certain tracts of the spinal cord, I get most valuable evidence from the cases which I produced in a paper read a fortnight ago before the Royal Medical and Chirurgical Society. The subject of this paper was a condition which may suitably be known as ophthalmoplegia externa, and of which the chief symptom is double-sided partial paralysis of all the ocular muscles. In the most marked cases, both eyelids droop and the eyeballs are almost motionless in the orbits. This affection (previously described by Gräfe) is due, not to disease of nerve-trunks, but of the nerve-nuclei, as made probable by its symmetry; it is progressive, as are certain forms of locomotor ataxy; and it may involve the optic centres and pass down the spinal cord. I cited, in the paper referred to, eighteen cases, and in about two-thirds of them there could be no reasonable doubt that it was due to syphilis. In the syphilitic cases, it is susceptible of arrest by the iodide, and without that remedy the disease is probably usually aggressive and in the end fatal. In many cases, it is mixed up with the symptoms of locomotor ataxy. Whether we prefer to say that it is a symptom of ataxy, or ataxy a symptom of it, does not matter in the least; the important fact is, that both result from a creeping process of disease leading slowly to degenerative changes in the spinal cord and mesocephalon, and having as their consequence various forms of muscular paralysis.

In near connection with this subject, I may refer to a group of symptoms which I described last year before the Royal Medical and Chirurgical Society, and which I believe to indicate disorganisation of the lenticular ganglion. They are: absolute paralysis of the iris, both as regards dilatation and contraction of the pupil, and paralysis of the ciliary muscle. The patient cannot read without strong spectacles, and his pupil does not act in the least. If these conditions be present without other forms of muscular paralysis, they are, I hold, to be explained only on the hypothesis of disease in this ganglion. Now, some of the cases occur in connection with syphilis, and are caused by it; and others have apparently no such association. In some cases, this

ophthalmoplegia interna is probably an early symptom of progressive disease which ends in locomotor ataxy.

A few general remarks may be here introduced as to the features in which syphilitic doubles differ from their prototypes. I have admitted fully respecting several special forms that not unfrequently the similarity is such that diagnosis is possible only by aid of the history and concomitants. This, however, is very exceptional, and, as a rule, minor features of difference are there, and may be made out by careful observation. It may be remarked, in the first place, that the syphilitic forms are seldom quite regular in their course; they are often more rapid in development than their parallels, and often we see mixed up the symptoms of several different forms of disease at the same time. Thus, we may find progressive muscular paralysis with the special symptoms of locomotor ataxy, and what should have been a pure retinitis pigmentosa may be complicated by disseminate choroiditis. In syphilis, the proclivity of all tissues to suffer favours these irregularities of development. The polymorphism of the secondary eruption of syphilis has long been well recognised. Next it may be asserted that a serpiginous tendency or habit is almost constantly more marked in syphilitic inflammations than in others. This is seen alike in the skin and in the connective tissue of the nerve-centres. If the disease be due to syphilis, it will, unless arrested by treatment, tend to spread faster and further than other forms. In the case of external parts, too, the liability to ulcerate, to suppurate, and to become phagedænic must be noted as special features of syphilitic inflammation, and to these we may add the liability to produce scars and to leave pigment-stains. Whenever the evolution of a disease is irregular, and the use of definite nomenclature thus rendered difficult, we may suspect that the malady is syphilitic.

It is time, Mr. President, that I should bring these remarks to a close. In doing so, I can only express the hope that they may have been at any rate suggestive. I have not aimed at proving anything, but have rather endeavoured to give hints as to the way in which I think we ought to work. If the conception of the state of the body left after an attack of syphilis, which I have tried to present to you, be the correct one, it amounts, I think, pretty nearly to this: that every specialised structure, every tissue, every individual cell, is left with increased and peculiar liabilities to disease. No one can tell where the morbid action will show itself, and we must expect it everywhere. Its special locality and form are probably determined by previously existing proclivities. The symptoms produced and the name which will be given to the disorder

will have relation to the structure and function of the organ or tissue which is attacked. Syphilitic maladies must be expected to be as multifarious as are the organs and specialised parts of organs in the body. The lenticular ganglion of the eye has been shown to be liable to disorganisation, and we know more or less definitely what symptoms would follow its destruction. But if this ganglion, why not the others in connection with the cranial nerves? why not those of the abdomen? why not the little ganglion impar itself, which, though so retiring in its solitude, and so unknown as to its special function, must still have its use, and whose loss, could we rightly recognise it, is no doubt felt in some degree? So of all the tissues in the body. It has well been remarked that the author of the proverb *ex pede Herculem* must have been a stupid fellow, for that he ought to have found a toe, or even a toe-nail, quite sufficient from which to evolve the Hero. So of our subject, it may be asserted that the proposition being granted that syphilis gives permanent proclivity to disease in all tissues, a good anatomist and physiologist might write the history of all its very various forms of malady without any clinical experience whatever. We have, however, in part accumulated such experience; we have seen Hercules' foot, and much more; and the task of inference as to the rest becomes so much the easier. I feel confident that the work before us—the discovery and accurate description of a large number of disorders which are of syphilitic origin—will be much helped if we take as our clue in it the general proposition that syphilis is an imitator. It is possible even that some assistance may be gained by work in the opposite direction; and that in some instances, having discovered first the syphilitic type of a special malady, we may find out by investigation the prototype hitherto overlooked.

I must correct what I have just said a little. A knowledge of anatomy and physiology alone would barely suffice to enable its possessor to foresee all that happens in syphilis. There must be some experience also in general pathology as well. Syphilis may claim position as a detector as well as a most skilful simulator. Under its influence, the hitherto latent proclivities of the individual are revealed. When epilepsy occurs from syphilis, it happens probably to a man predisposed to epilepsy; and the same of locomotor ataxy, and the like. The reason why one man in the secondary stage of syphilis shows a scaly eruption, and another a bullous one, is no doubt that peculiarities existed in the individuals which gave a tendency to one or the other form of morbid action. Syphilis in its actions upon the cell-elements of the body

deranges their vital stability and unbalances their forces. It may be said to put a light to a train. The precise character of the explosion will depend upon the quality of the powder and the nature of the surrounding materials.



ADDRESS IN SURGERY,

DELIVERED AT

The Forty-ninth Annual Meeting of the
British Medical Association.

Held in RYDE, August 9th, 10th, 11th, and 12th, 1881.

BY

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ADDRESS IN SURGERY,

DELIVERED AT

THE FORTY-NINTH ANNUAL MEETING OF THE
BRITISH MEDICAL ASSOCIATION.

MR. PRESIDENT AND GENTLEMEN,—As we are most of us fresh from a long week's carnival of medical science, I feel sure that you will not desire that I should on the present occasion address you on any subject directly connected with surgical practice. The very successful and industrious Congress which concluded yesterday has had its numberless Section meetings, in which most of us have taken some part; and, if I do not misinterpret the general feeling, it is a longing for a little rest as regards those matters in the discussion of which we have so recently and so fully engaged. I purpose, therefore, to employ the hour which you have done me the honour to entrust to me, in bringing under your consideration certain general topics having reference to surgical ethics, surgical education, and, lastly, the best means of advancing the clinical knowledge of disease.

Five-and-twenty years ago, the surgeons of the metropolis were alarmed, I might almost say scandalised, by a proposal to open a hospital for the treatment of stone and diseases of the bladder. A memorial was got up, and signed first by the ever respected name of Brodie, and then by almost the whole of those then connected with the general hospitals. This memorial severely condemned the multiplication of special institutions, and with particular vigour denounced the one in contemplation. Amongst the names at its foot, mine may be found. I am not ashamed of having signed it then, but I unhesitatingly say that I would sign no such memorial now. Nor can I believe that many of those who then did so would now differ from my present conclusions. Not that the facts then stated have been materially changed, but others have been added. Year by year, we have seen more clearly that, in large communities, special hospitals will develop, and that it is beyond the power of the profession to prevent them. But we have seen more than this: we have seen that they are clearly a gain to the public; not an unmixed gain—for what gain is unmixed?—but still a gain. Even at the time when the memorial to which I have referred was written, it was a matter of necessity to

admit that diseases of the eye constituted an exception, and some thought that orthopædic institutions should also be permitted. Since then, I suppose that those for diseases of women, for diseases of children, and perhaps for diseases of the skin, have justified themselves.

The final triumph of ovariectomy did not result from any new discovery. There had been many pioneers. The instruments and the modes of practice which had been devised by others were those which were still employed; but these, in the hands of able surgeons, who gave their whole time and energy to this one subject, and who had, above all, the power of excluding from their special institutions those sources of evil from which a general hospital can never be wholly free—attained for this operation its present proud position. Let us note carefully the two elements of success. It may be that Listerian or some equivalent precautions may in the future make the wards of a general hospital as safe a place in which to open the peritoneal cavity as is a private home or a special hospital; but, even if this be done, they can never supply the familiarity with detail which comes only from constant practice. We must never forget that our profession exists, not for the benefit of ourselves, still less of any special class amongst us, but for our patients; and that its institutions must be so managed, or so modified, that they shall best serve their permanent interests. Nor can I think that there is any real difficulty in making the two coincide. Let the profession set its face, not against specialisms as a whole, but rather against those institutions which are conducted in a narrow spirit. Let it insist upon open elections of officers, free admission of students and practitioners; above all, let it encourage the formation of special departments in our general hospitals, since it is clearly here that they can best be made useful in the education of the student.

These general principles of conduct being granted, I would not oppose the beginning of any speciality, however detailed. Already the comprehensive department of eye-diseases is submitting to some process of natural subdivision, and it will not be long before we shall have in our large cities those who devote themselves chiefly to operations on the eye, those who attend specially to its diseases properly so-called, and those who rectify its congenital or acquired defects by means of optical aids. It by no means follows that because a man is a good operator for cataract, he will be equally familiar with the details of astigmatism. It is impossible to question the fact that the great discoveries in ophthalmology have been made by specialists, and sometimes by those who had devoted their attention chiefly to special branches of that department. Let it be understood that I am not arguing in favour of the promotion of subdivisions of surgical labour, but rather to the effect that, when they come naturally, we should not oppose them. Let us have charity for the various motives by which different men are urged to different courses; let us hold high our standard, inscribed "for the good of all", and allow to natural energy a full and free development. Let us not waste our time in opposing, but seek rather to employ and use.

I rank amongst the gains from the detailed cultivation of special branches of medical pursuit, that it has a definite tendency to destroy specialisms as such, and add their conquered territory to the general possessions. Whilst it does this to a large extent, it also at the same time creates new departments before unthought of, into which science pushes its way, and gives its help towards the mitigation of the many disabilities of man.

Witness what has been done in Ophthalmology; how discoveries one after the other have been made, which have been at once added to the knowledge of the general physician. Note how the ophthalmoscope has taken its place with the stethoscope as an indispensable aid to the practitioner in the diagnosis of disease. The knowledge of diseases of

the eye has indeed rapidly attained in the profession at large, and not alone amongst specialists, a very high degree of perfection. I do not hesitate to assert that there are few departments of practice so well and widely appreciated, and few which more definitely attract the attention of students. Nor do the gains to the public from this increase of general knowledge represent the whole gain; for the study of eye-disease must be claimed as especially useful as a training in the art of precise observation. No student masters it who does not become in so doing much better fitted for other fields of clinical research. This study is indeed in relation to other branches of surgery almost what mathematics is in general education. We have but to reflect on such facts as these, and next try to recall what the ignorance of eye-disease was less than a century ago, and what the position held by the oculist and the spectacle-vendor, and we shall be able to estimate a part of the debt which we owe to specialists.

There was a time when Diseases of the Skin were regarded by the higher class of surgeons with feelings almost allied to contempt. They were repulsive alike in the portrait, and in the person of the patient. They required no operations; and a knowledge of the use of arsenic, and of the constituents of a few ointments and lotions, was held to be all that was needed for their treatment. Then followed a period during the early cultivation of the speciality, when to outsiders it seemed as if it offered only an arena for endless wrangles as to schemes of classification, and for ingenuity in the devising of new names, and affixing to them a countless variety of unclassical adjectives. I am not quite sure that this era has even yet wholly passed away. It may be that there yet lingers a feeling of prejudice to this speciality amongst some who should know better. With the majority, however, and especially amongst our rising pathologists, a wholly different conviction is rapidly superseding such sentiments. We are learning to care little about names, and to seek knowledge as to the nature of things; and those who do this, in earnest, rapidly find out that, of all the departments of pathological and clinical research, there is none which offers such rich and varied attractions as does Dermatology. The simple facts that a morbid process in the integument is, from its beginning to its ending, exposed to view, that the aid of the lens may be brought to bear upon it while yet *in situ*, and that the histologist is in very many instances not obliged to wait the death of his patient before he is allowed to gratify his thirst for knowledge of ultimate details, largely justify my assertion. But it is not upon them solely that I rely, when I assert that diseases of the skin ought to be regarded as fundamental in professional education. Many illustrations which they afford of the changes in vascular supply and its results; of the influence of the several parts of the nervous system in the production of disease; of the laws of inheritance; of the numberless varieties in the inflammatory process in connection with diathesis, idiosyncrasy, and special forms of poisoning; are varied and instructive, far beyond those offered by any other department. Above all, we find in dermatology the most remarkable and conclusive proofs of the direct connection between morbid causes and their effects. I cannot doubt that, possessing these advantages, skin-diseases must in the future be accepted as not only of the utmost interest for themselves, but as invaluable to the physician and pathologist in the elucidation of the phenomena of diseased action, as met with in regions and organs less open to inspection. I by no means claim for this insight into the educational value of a knowledge of skin-diseases that it is wholly of modern growth. It was recognised long ago by the Paris physicians; and the work done by Anthony Todd Thompson, by Jenner, Gull, and above all by Addison, attest to its partial recognition amongst ourselves. But its growth is only recent, and never had Dermatology such

an army of workers who are not specialists as it at present claims amongst the younger physicians and surgeons, not only in London, but in all parts of the world. In their hands, it is certain before long to assume that foremost place which I claim for it as its natural position.

Let us remember, respecting a large majority of the maladies which it has been the fashion, in some sort, to stigmatise as "skin-diseases", that nothing is more certain than they belong mainly to other departments of pathology. Herpes is a form of neuritis which happens to display some of its symptoms on the skin. Morphœa is an affection of the vaso-motor nerve, leading to changes in bones, muscles, and joints, as well as to those which, from occurring externally, first attracted attention, and still almost monopolise it. In the study of ringworm, we may engage, if we will, in the most interesting investigations as to the laws of life in minute vegetable organisms. Leprosy offers us a dietetic problem of at least equal interest with those which concern gout and rickets. In psoriasis and its allies, we study a heritable peculiarity of health or of skin which shows its effects through a whole life, and is influenced for better or worse in ways that are curiously instructive. Who can doubt the power of drugs who has seen arsenic cause herpes or cure pemphigus? The polymorphism of syphilis is a fact to claim the permanent wonder and interest of all who are instructed in pathological speculation.

We listened, with delight, last week, to the eloquent words in which, in his opening address, Sir James Paget enforced the duty of charity amongst ourselves. It is in this spirit, I think, that we should meet the various questions which open out in connection with the medical education of women; and that, also, as to consultation with those who, whilst educated amongst us, have openly professed their adoption of peculiar doctrines. I fear that I am here venturing upon ice that is very thin indeed; and I must proceed either with great boldness or extreme caution. I shall prefer the latter. Let me say, then, that the profession of medicine always has offered, probably always will offer, peculiar attractions to those who, with weak principles, and still weaker consciences, desire to make profit by trading on the credulity of their patients. The thing is so exceedingly easy to do. Our real knowledge of disease in many of its departments is very vague, and our knowledge of therapeutics still less certain. There is room on all sides for differences of opinion, and scope for the introduction of new theories and the employment of high sounding epithets. The fatal facilities thus afforded to the charlatan have naturally made the well principled professors of physic very vigilant in guarding our ranks against the introduction of quackery. We wish to be honest, and we wish to associate with none but those who are so. There lived, now more than a century ago, a talented and learned enthusiast, who thought—sincerely, I have little doubt—that he saw his way to an immense reform in the use of drugs. That reform was needed, we all admit. He noticed a few common facts, such as the marvellous subdivision of which odorous substances are susceptible; the change of effect, or even reversal, which accrues from change in the dose of a drug; and that, respecting some few medicines, it was true that they seemed to produce in large doses just what they tended to repress in small ones. I am not here to apologise for Hahnemann. His facts were few, his reasoning illogical, his ignorance of the natural progress of disease and its tendencies to spontaneous recovery, such as would be utterly disgraceful in the present day. Whatever foundation we may grant for some of his theories, he certainly pushed them to the wildest lengths.

If, however, we may find theme for marvel in the presumptuous self-sufficiency of this would-be reformer, I do not think that we need seek

far for the explanation of the success of his teaching. It inculcated faith in drugs, but it changed their form, and gave us cleanly globules and tasteless fluids instead of the bolus, pill, and potion. It supplied a theory of cure, as well as its means; and to the intelligent, but, at the same time, not specially trained, its theory sounded at least as good as those of orthodox physic. The love of novelty conspired with a cheerful faith in the possibility of progress, and with delight in escape from the disagreeableness of the old methods, to draw converts to the new creed. Those converts were not the ignorant, nor were they the poor. No wonder that some from our own ranks should have thought they saw their interest in adopting the new method, and equally little that most of those who observed their conduct held the motives of the man who put "homœopath" on his door to be low and self-seeking. In nineteen cases out of twenty, probably the verdict was right; but when the fiat went forth that a homœopath must be either a fool or a knave, I doubt whether the modesty of nature was not somewhat overstepped. There are fools and fools, but we are guilty alike of unkindness and unfairness if we widen that disrespectful epithet over much, and apply it too freely. There is such a thing as a combination of weak power for the estimation of facts, with enthusiastic optimism as regards possible progress, which, whilst it in no degree establishes a claim to wisdom, yet scarcely brings its possessor into the category of fools. Amongst the laity, of those who became homœopaths, most were of this class, and some, probably, of those who seceded from our own ranks.

I fear that it may be thought that I am travelling very far from the proper subject of an address on surgery. I also much fear that I may be misunderstood. What, it will be asked, has homœopathy to do with surgery; and why introduce the question of consultations with homœopaths by such a lengthy prelude? Now, it is precisely because homœopathy has nothing to do with surgery that it becomes of importance to us to settle the question in dispute. The circumstances of Lord Beaconsfield's illness are fresh in the memory of us all. We gave honour to Sir William Jenner for his stern and manly refusal to have anything to do with what he thought quackery; but we also sympathised with Dr. Quain in his perception, that the occasion had arrived for the sacrifice of sentiment and the performance of a disagreeable duty. What particularly struck me in the transaction, and what constitutes my chief reason for mentioning it now, was the reason alleged by Sir William Jenner why he could not meet Dr. Kidd. It was not that he felt compelled on principle to decline all intercourse with the heterodox, but that the patient could not possibly be a gainer by a conference between those who held such different opinions respecting the principles of therapeutics. It is clear that, had it been the aid of a surgeon that was needed, no such reason would have been valid. Homœopaths have not as yet succeeded in developing any new system of surgery. The knife and the catheter are the same to them that they are to us, and are used on the same indications. I never myself wittingly consulted with a homœopath; but I believe that I have, without knowing it at the time, several times met them, and I never yet encountered the slightest difference of opinion. The surgeon, then, cannot possibly feel that he is in any way serving the interest of the patient when he refuses to meet his weak-minded doctor. On the contrary, it may easily be the fact, that he knows that it would be the greatest possible kindness if he would go without an hour's delay. To Boycott a quack on principle is one thing, to attend to the interests of the quack's patient may be another. Hence the duties of surgeons in this matter, and especially of those engaged in consultation practice, have always been very difficult. The obvious incongruity which exists in the case of a physician is not present to the surgeon; his temptations are both more frequent and

stronger, and his sources of inward strength are fewer. He refuses neither for his own good, nor the patient's good, but in obedience simply to professional rule. With a few exceptions, this rule has been, I believe, honourably upheld by the consulting surgeons of England. There have, however, been some exceptions, and there have been difficulties and annoyances without number. Now we cannot possibly, in our profession, have one rule for the peer and another for the tradesman. I avow my deliberate conviction that Dr. Quain, and those whose counsel he sought, interpreted the obligations of our profession correctly. We enjoy a law-established monopoly in the art of healing, and we must be very careful how we stint or refuse our services when they are demanded. If, in consultation, it be found that the opinion of the consultant is not that of the consulter, and, if the latter be not willing to waive his own, then the proper course of conduct is clear. But, such inability to act in concert surely should not be assumed on light grounds, or hearsay evidence. Unless the previous knowledge be very special, it should be established at the bedside in each individual case. I am speaking of formal consultations only, not of social intercourse. We know well how to accord and refuse professional honour. If a man be guilty of non-professional conduct, we can blackball him at a society, and avoid his company in social life; but we enter upon a course of conduct which needs a wholly different justification if we refuse to meet and confer with him, when the life of a third person is concerned. Here I confess that it seems to me that the claims of the public should stand first, and that if a man's name is on the *Medical Register* we ought to meet him, so long as the consultations result in that which we deem most for the patient's advantage. Whenever they do not, our duty is clear, and we should readily know how to perform it. Many advantages, I think, result, if we were to leave with the licensing bodies the responsibility of decision, as to who are to be admitted to the privileges of formal consultation, and who excluded. To do so would save at once much loss of time and of temper, and avoid frequently recurring complexities. It would encourage honesty and openness of conduct, and remove temptation to the secret perpetration of that which is known to be against the professional rule. But, above all, I would urge that it seems to be almost a matter of justice to the third party, our patients, who have surely a right to assume that, when a duly qualified man is employed, they can obtain, in consultation with him, if wished, the aid of any other, who is engaged in the public practice of his profession. That we run the risk of fostering homœopathy by according to its disciples the courtesy of consultation, I do not for a moment believe. It has hitherto been fostered by opposition. Let us have more confidence in the vital energy of truth, and let us venture to let the wheat and the tares grow together till the harvest. We believe that its principal theory is absurd, and much of its practice ridiculous; but, at the same time, we are prepared to admit that gleams of a fruitful suggestion may be occasionally discerned in its discussions, and we can surely afford to leave it as a whole to itself, and let it develop to its natural end.*

I will pass to a less distasteful topic, and proceed to make some suggestions as to the possible improvement in our methods of clinical teaching. It must be noted, in the first place, as a great defect in our English system that it makes no provision for retaining the services of

* These paragraphs concerning consultation with homœopaths did not form part of the address, as given at Ryde. They had been written out for delivery; but on the evening before, I learnt that the same subject had been far more ably dealt with by Dr. Bristowe in his Address on Medicine. The explanation of our having selected the same subject will be easily seen. Dr. Bristowe dealt with the whole subject, whilst I have spoken only as to the inexpediency of continuing to refuse formal consultations. I have reinstated these paragraphs, in consequence of a general request that they should appear when the address was printed.

good clinical teachers. It trains them, and then casts them adrift. The early period at which our surgeons and physicians retire from hospital work is a matter of amazement to our foreign *confrères*, and few can doubt that the evil is on the increase. Either by the bribe of private practice or the gentle compulsion of a retirement rule, we induce our best teachers to desist from work in public just at the time when their services are of most value to the student. The names of Paget, Jenner, Erichsen, Gull, and Bowman will occur to us all in illustration of what I assert. Not only do our customs make the early retirement of successful men a matter of necessity, but they often cramp their usefulness to the student during the latter part of the period of their tenure of office, and also hinder their services to science in other directions. Success in our profession means absorption of the whole time and whole mental energy in attention to private practice. No individual surgeon can help himself; he is in the meshes of the social net, and escape is hopeless. It is a change of custom which must effect the reform, and surely such a change is urgently required. It might come, I think, very suitably in some such form as this. Let there be constituted a rank of pure consultants, whose fees shall be so modified as to enable them to make the same incomes that they now do with a third of the personal labour. Let election to such grade be by their respective colleges. Let us no longer leave the time and the energies of our foremost minds at the mercy of any wealthy man who, however trivial may be his ailment, determines to have "the best advice", just as he would seek the most expensive jeweller or buy the most costly wine. Such men are too valuable for that. By the plan which I suggest, leisure would be left to them for study and for teaching, and they would be retained in their proper sphere of public work. Consultants of the class I suggest, having restricted their spheres of observation of disease in private, would find it necessary, in order alike to sustain reputation and keep abreast with progress, to retain their public appointments, and to continue to teach. The gain would be a great one to medical education, and also, I think, to the public interest. The evil to which I refer is an increasing one, and it exists to a far greater extent in London than in any other capital.

A large part of the education of students may very suitably be done by young professors. Age adds but little to the ability to deal well with the facts of anatomy and physiology, and it may even detract from it. When, however, we come to the knowledge of disease at the bedside, and the correct estimation of its various symptoms, then experience must tell. Without it there can never be that trust on the part of the student or that free power of illustration from facts which have been personally observed, on the part of the teacher, which are essential to success.

Yet it is precisely this clinical teaching which is of most importance to the student, and in which, if I mistake not, our modern system is most at disadvantage. Apprenticeships have been abandoned, and few that remember their inconveniences can wish to revert to them. Yet they had their uses, and those not trivial ones. They unquestionably often secured to the student a clinical and practical training which is now but too often missed. Permit me to make for your consideration a suggestion of a plan by which possibly most of their advantages might be retained, and others which they did not always possess secured. Let us endeavour to widen the basis of medical teaching, and to enlist, as responsible partakers in this all-important work, a large section of the profession. This might be done if our examining boards were to recognise as private teachers all fellows of their respective colleges, all possessors of diplomas of the higher class, all medical officers of hospitals and dispensaries, indeed, all who could bring proof of the possession of special opportunities or qualifications for clinical teaching. Let it be required of every student that he should

bring, in addition to those now required, certificates from two, three, or more of these registered teachers that he had been—in a real and *bonâ fide* manner, under their personal supervision—instructed in practical matters, and that he was in their opinion qualified for a diploma. Make such certificates necessary, but let them not modify in the least the curriculum of the school or the test-examination. To prevent these certificates from being signed in a perfunctory manner, let it be the rule to inform those who signed them of the results obtained by their *protégés*, and if in the course of a five years' period the proportion of rejections should be more than an average, let the name of the teacher concerned be for the next five years removed from the list of those privileged to sign. This plan would, I think, work well in many ways. It would add to the value of the higher diplomas, and increase the number of those who seek them. It would encourage in those registered as private teachers the endeavour to keep well instructed in the knowledge of the day, whilst its advantages to the student—by bringing him into closer personal contact with those interested in his success—are obvious.

How various are the qualifications which are to be desired in the medical practitioner. He should be a gentleman of good manners and address; skilled alike in the principles of biology and in the knowledge of character; able to visualise at short notice the details of human anatomy; and he should carry in his memory, ready for prompt use, the best recipes for a thousand varying forms of ill-health. If we measure his responsibilities by the possibilities of his usefulness and the risks of his failure, they are very great indeed. It may be true that the greater part of his duties are routine and easy of performance, but he may, at a moment's notice, be called upon to deal with the unusual and difficult. Regarded from this point of view, the amount of knowledge required of him is enormous—greater probably than that necessary in any other avocation. It is, further, steadily on the increase; every fresh discovery brings with it something fresh for the general surgeon to master and retain in memory. Hence many of the increasing difficulties in surgical education; hence unquestionably the increasing ratio of plucks to passes at our examining boards. It is not that examiners are more strict, students less able, or teachers less zealous; but simply that the thing to be taught has grown in bulk, and become year by year more and more difficult of attainment within the allotted period. There is nothing whatever to discourage us in the fact, much, indeed, to tend in the opposite direction. But we must boldly meet the changed and ever changing circumstances. An extension of the period of study, a well considered limitation of its subjects, and lastly, a careful development of its methods, are the three measures which severally suggest themselves. The last is, of course, approved by all, and is too obviously desirable to need comment; but concerning each of the others there is room for much debate. As to the extension of the compulsory period of study, such proposals may, I think, be dismissed with the remark that the practice of liberal rejection of candidates imperfectly qualified really amounts to the same thing, and attains its end with more justice to the diligent and able. In the future, it may perhaps come to be considered a great credit to pass the first time, and no disgrace to be referred. Careful men, appreciating the necessities of the case, will probably voluntarily lengthen their period of study. Were the period compulsorily fixed at five or six years instead of four, the careless would still, as now, idle till near the end of it. I cannot but think, therefore, that the practice of early examination, with its necessary result of many rejections, works on the whole better than would any which should make an indiscriminating demand for longer time.

It is impossible not to regard without the utmost jealousy any proposal that the subjects of professional study should be reduced. So far from its being desirable to strike out botany and comparative anatomy, we might prefer to see added, if possible, a good knowledge, not only of the anatomy and functions, but also of the diseases of both plants and animals. It is from a broad education in these directions that we may hope for future advance. Having said this, however, we must hasten to admit that a large majority of the profession are to be trained, not so much as biologists, nor even as pathologists, but as practitioners. In our surgical education there is much that is valuable, very much indeed that is of the greatest possible interest, concerning which it still cannot be said that it is essential. It is certainly the duty of both teacher and examiner to draw a strong and clear distinction between essential and non-essential acquirements. Howsoever the latter may fare, the public has a right to demand at our hands that the former shall be in as complete possession as possible. It is no comfort to the glaucoma patient, who has been treated by lotions and leeches until he is blind, to know that his surgeon is a good anatomist; nor will the most excellent knowledge of histology avail to save a practitioner from something in its nature not unlike manslaughter, who believes that he ought to wait for tympanites and stercoraceous vomiting as the chief symptoms of strangulated hernia. It is common sense and practical knowledge of common things that we mainly want. I well recollect an anecdote, which was told to me when a boy, respecting a smart young farm-labourer in my father's employ. This young fellow had incurred the wrath of a half-witted young woman in the village, who, in revenge, said of him: "He can whistle fairly, and he can sing pretty well, but he can't plough straight". This home-thrust so rankled in his breast that his accomplishments became annoyances to him, and he finally left the neighbourhood, unable to bear up under the frequent reminders as to what daft Meg had said. The distinction between the essential and the ornamental was here so strongly emphasised that, although the latter was not in the least depreciated, it stood as less than nothing in comparison with the former. So surely it ought to be with us. Respecting the essential, examiners might perhaps do well to leave nothing to chance, but, regardless of time, to make sure, so far as is possible, that the candidate possesses a really sound knowledge of them. It by no means follows that a candidate who knows what to do in traumatic gangrene is equally up to the mark in reference to purulent ophthalmia; nor does a good knowledge of the latter imply ability to treat a case of prostatic retention with success. Yet these are all equally essential, and they stand pretty much in the same relation to the duties of a surgeon as does straight ploughing to a farm-labourer.

We may reasonably hope that improved methods of instruction, and the application of common sense to our plans of teaching, will do away with the need for any material curtailment of the scope of study. The means provided for the education of students may be classed under three heads: Teachers, Books, and Museums. Of the two former I do not propose to say much, but the last is an attractive topic which I cannot pass. The change which followed on the introduction of printing, in reference to the value of oral instruction, has often been the subject of comment. At the present time this change is complete, and no discoverer or propounder of new doctrines would ever think of bringing his observations before the public in any other way than by the aid of the printing press. He does not expect his hearers to come and listen to his professor's lecture, but he embodies his opinions in a book, and then sends them broadcast over the world. If, in the first instance, he reads a paper or gives a lecture, it is that it may be printed afterwards. The professor of the present day is, for the most part, an exalted develop-

ment of the tutor, and his duty is almost as much to ascertain that his pupils do really learn from books as to teach them from his chair. Few, indeed, are there who can attract hearers from outside their allotted classes. Nearly all have themselves published books, and their success, in nine cases out of ten, depends far more on their willingness to adapt themselves to the existing state of things as regards students' requirements—to advise, supervise, and question—than upon their abilities in the lines of original research. For the latter, the vocation is elsewhere. I am speaking, of course, of the professors and teachers in our colleges and schools of medicine. They have become the expositors of books, not of their own original and unwritten opinions. It is, I think, not improbable that another development is at hand, which will yet further diminish the importance of our chairs, and of oral instruction in general. I allude to the creation of Students' Museums. The museum, hitherto, has existed chiefly as an appendage to the chair. Our examining boards have required that teachers of anatomy, physiology, and pathology, should possess or create a museum of specimens from which to select illustrations for lectures. In many instances, these collections have not even been made accessible to students, and in none, until quite recently, has encouragement been given to the student to regard the museum as a place in which he ought to work. In one of our large European capitals, possessing a flourishing medical school, there was, I believe, until very recently, and I am not sure that things are materially changed now, no museum which a student could enter without the most troublesome formalities. In several other continental cities possessing medical schools, the creation of a pathological collection, worthy the name of museum, is a matter of exceedingly recent date. I claim it as a distinguishing feature of our own country, one, however, which I willingly share with our Scandinavian relations, that our museums have for long been numerous and good, and I further assert that it is amongst the most valuable proofs of life in the scientific spirit which we can show that they are constantly growing and improving. In no country, however, have we as yet seen the full development of museums as means of medical education; and, if I am not much mistaken, these institutions are destined in the future to assume an importance of which we have as yet scarcely dreamed. Well managed, it will be found that the museum may be made to combine the advantages of dissecting-room, ward, lecture-theatre and book in one, and that it can supply permanently, at all times, and to all comers, opportunities, which are for the most part accessible elsewhere only at special times and to a privileged few. I do not mean that dissected preparations or models can ever supersede or rival work done *propria manu* with forceps and scalpel, but they may be made to assist it, to prepare for it, and to supplement it with most excellent results. Hitherto our museums, whether medical or in connection with general knowledge, have been far too miscellaneous. Huge crowded collections of material, some of it of the greatest value, and some of it of very little, have jostled together in a more or less orderly kind of confusion, through which only the well instructed can find their way. They have been, by the majority, visited rather as places of wonderment, or perhaps of bewilderment, than for systematic instruction. Now museums ought to be as legible as books; and, when they are made so, they will be eagerly read. The very most that can be said of our very best, as yet, is that they have approached somewhat to the character of cyclopædias, from which fragmentary information upon all sorts of subjects may be obtained by those who know how to search for it. For students' purposes, something of a different kind is required—something much less voluminous, at once more concise and

more consecutive: in fact, to pursue the comparison, more of the nature of a *handbook*. A students' museum should contain those things which a student wants, and those only. These should be well arranged, with plenty of space; and well labelled, not merely with a name, but a description. There should be nothing to distract attention, and everything to favour study. Anatomy should be illustrated by dissected preparations, casts, drawings, and diagrams; and these should be kept in juxtaposition; and from anatomy to pathological change the steps should be direct and clear. Side by side with the normal joint should be the diseased joint; with the specimens illustrating the precise position of the epiphyses, those showing their detachment by violence. No knowledge should be taken for granted in the learner, and as far as possible everything should be demonstrated. I must not go further into detail. Let me conclude what I have to say on this topic by the remark that we ought to have museums for educational purposes, distinct and wholly separate from those which are designed as magazines of facts deposited for the use of original investigators. The two objects are different; and not unfrequently that which is essential to the completeness of the one is useless and cumbersome to the other.

In speaking of students, hitherto, I have been thinking of those who have not passed their examinations; but it is the boast of our profession that it possesses countless students of another grade, who have no longer the fear of the examiner before them, but who recognise the fact that even a lifetime is too short to acquire a fair familiarity with the facts of pathology, and who, however long their lives may be, will remain in the position of pupils. For these also museums ought to be provided; and here, again, I make bold to assert that nothing in the least adequate to the wants of the case has as yet been attempted. Our grandsons, if not our sons, will smile at the *dilettante* manner in which we have been content to hunt for the truth in matters of clinical research. We so often treat disease—at any rate, in its less common forms—as if it were sent merely that we should write papers about it, and discuss its nature in more or less detail, and with more or less seriousness, according to the humour of the hour. I do not speak now of the more practical section of the profession—men engaged in the daily and hourly discharge of routine duties, in the conscientious endeavour to apply well-known rules to the treatment of disease, and only exceptionally concerned in the pursuit of new knowledge. I speak rather of the more ambitious amongst us—and, thank Heaven, they are now very numerous—men who attend societies, compile statistics, collect specimens, and write papers. Of these I assert—may I be permitted to claim for myself a humble position amongst them, and say of *us*?—that we are far too ready to yield to the temptation of thinking that disease was made for the physician, and not the physician for disease. We investigate it in the same spirit that an amateur geologist brings to his problems—as a thing which may agreeably exercise our ingenuity and train our minds, upon which we may perhaps base our reputations, and out of which there may perhaps come some good for mankind. We claim to lay aside our work when we are tired of it, and to vote its too urgent pursuit a bore, forgetting that such investigations are to us a matter of the most urgent professional duty, and to our clients one of life and death.

That I may not seem to loose an unaimed shaft, I will take an example, and it shall be one in which almost the whole of the English profession to some extent is concerned. The museum of the Royal College of Surgeons, the Hunterian Collection with its fifty years' additions, is without a rival in the world. It is a noble museum, and has been nobly cared for by a succession of curators who have made their names

famous in science by work done within its walls. It may seem a bold thing to charge against an institution so foremost, that its arrangements are inadequate to the wants of the present day. Amongst the wants of the medical profession at the present time, and amongst those wants which alone a national museum is fitted to supply, is the fullest possible information respecting the symptoms of disease. It is not enough that we illustrate its final results, that we keep in bottles or otherwise the documents which demonstrate its ultimate conditions: we need a pathology of the living as well as of the dead; and everything that human contrivance can do to elucidate this should be attempted. Here we must mark a huge hiatus in the arrangements of our College. There is little or no endeavour to illustrate the effects of either disease or injury in the living, and an exceedingly meagre attempt to show what has been done in reference to instruments for surgical treatment. The modeller's art, of which such beautiful examples from Paris have recently been shown to us in the Congress Museum, is at our College "unknown and like esteemed". I must correct myself; for there is, in a topmost gallery, a small collection of models, the gift of our present President. These, however, are only a little series selected from the magnificent array of similar objects to be seen in the St. Louis Museum at Paris. It is imperfect and ill-shown, and the College makes no effort to add to it. A museum adapted to the wants of the practitioner should supplement the hospital, whenever possible, in the display of the outward characters of disease. Nothing in the whole range of diseases of the eye and skin or other external parts, nothing that a speculum can show, or a modeller delineate, should escape it; the rare and the common should alike be there, and the practitioner should be able to resort to its galleries when in doubt as to diagnosis, or desiring to recapitulate his knowledge, with confidence that he will there see all that can be shown. Nothing less than completeness should be the aim, and from all sources copies should be procured where the original cannot be had—the photographer, the modeller, and the artist being employed without regard to expense.

If a surgeon were now to go to our museum and ask to see models which would help him in the diagnosis of the different forms of chancre, or in the recognition of such sores on unusual parts of the body, I fear that he would be disappointed. He might find an unequalled collection of pre-historic skulls, and the skeleton of a splendid whale, but little or nothing in reference to the practical object of the diagnosis of surgical disease. Yet, in proof that it is possible to give such aid, I again appeal to the St. Louis collection.

Let it not be thought that I speak in disparagement of anthropology or comparative anatomy. It is a proud boast of our profession that its members have been foremost in these pursuits, and long may it be sustained. In their liberal cultivation, we follow the example of the founder of our museum, and keep up its most cherished traditions. But surgery has widened much since Hunter's time, and its special cultivation now asserts claims which did not then exist. These claims are, I cannot but think, primary in such an institution, which, whatever it leaves undone, should first attend to the duty of giving all that it can possibly give to the elucidation of human disease and the means of its relief. I cannot, however, believe that a liberal development in the new direction would necessitate any curtailment in the old. Let but a proper appeal be made to the profession, the public, and the Government, and means would surely be forthcoming which would enable our National Museum of Surgery so to develop itself, that no surgeon should ever spend a day in London without a visit to its collection, and none should pass through its rooms without obtaining information which would be of the utmost value to his patients.

There is yet another kind of museum which is, I think, a desideratum, and which I have no doubt the future will possess: I refer to a Museum-Hospital, in which living persons, the subjects of chronic and incurable diseases of an unusual kind should be collected and encouraged to remain, with, of course, every attention to their comforts, for long periods; every facility being offered for their inspection by all members of the profession. From such hospitals I would wholly exclude common cases, such as are useful, nay essential, in the training of the student, and would collect only those likely to be instructive to advanced practitioners. Each case should be carefully studied by competent authorities, and described in accessible catalogues. A visit to such an institution would be invaluable to the man engaged in busy general practice, and its growing records would become rich mines of information to the clinical investigator. Rare maladies are not to be regarded as mere objects of scientific curiosity, but should be utilised to the utmost, and made, if practicable, familiar to all; for in them often lies the key to the interpretation of other pathological phenomena which are common enough. Let us cease from dilettanteism, and try to economise our resources. When we do so, surely we shall find that there is a better way of dealing with examples of Myxœdema, of Addison's disease, of Charcot's joints, and Morphœa, than by relegating them to the wards of an union asylum, where they will be seen by none. I mention these merely as examples; there are many others concerning which it is equally true that, if opportunities were afforded for their collection together, under conditions of facility for research and inspection, great help would be given both to medical education and to the progress of clinical knowledge.*

I should be very sorry if what I have said, perhaps too plainly, as to what appear to me to be defects in our existing arrangements for the promotion of surgical knowledge, should have left the impression that I am in any degree a dissatisfied complainer. I hope that I yield to none in thankful appreciation of what has been accomplished in the past. But surely the fulness of the harvest which we have been permitted to reap, should prompt us to increased diligence in putting in seed for the next. We have every reason to feel encouraged and hopeful, but let us not allow the sentiment of confidence to lull us into sloth.

The chemist and the empirical seeker after new drugs may, I suppose, share the pleasure which must come from the knowledge of how iodide of potassium has made curable a whole phalanx of maladies before hopeless, and not the less full of misery because often accompanied by the bitterness of self-reproach. The operating surgeon may remember the triumphs of ovariectomy, which has restored in health hundreds of mothers to their families. If we could bring together in one place those who, thanks to the ingenuity and industry of Von Græfe, have been by iridectomy saved from blindness through glaucoma, and are now enjoying the blessing of sight, they would crowd this large hall, and leave no standing room. The abstruse optical researches of Young, Helmholtz, and Donders have borne fruit in the fact that thou-

* An experimental attempt in this direction was made by the Committee in charge of the Temporary Museum organised for the recent Congress. It was, I believe, considered successful; at any rate, although conducted under great disadvantages as regards want of room, it was largely visited. Every morning at a certain hour a selected number of living patients, illustrating for the most part very unusual maladies, were brought together for examination, brief explanations being given by those who brought them. In this way opportunity was afforded for the simultaneous inspection of groups of each of the following maladies:—true leprosy; severe gout, and mixed gout and rheumatism; myxœdema; osteitis deformans; spondylitis deformans; lupus erythematosus; primary muscular atrophy; morphœa; syphilitic teeth; xanthelasma; and many others.

sands all over the world, whose sight was comparatively useless, now enjoy it in almost full perfection. The purely practical man may rejoice in remembering how much Sayre's jacket and Martin's bandage have done, and are daily doing, for the mitigation of suffering and the cure of diseases which render life a burden. The application of the germ theory to the treatment of wounds has, I doubt not, had for one of its results, amongst many others, that at the present moment there live, scattered in very distant places, many thousands of able-bodied men, the fathers of families, now earning their children's bread, who but for it would long ago have been in their graves. It is true that we as yet see no hope of a cure for cancer, but the pathological doctrine, which is rapidly gaining ground, that many forms are local, and that the pre-cancerous stage should be vigilantly recognised and vigorously treated, is already saving many from becoming its victims.

A few weeks ago, visiting a renowned cathedral, I found inscribed on its floor at long distances apart, three remarkable words, *Credo, Spero, Amo*. I am not ignorant of the special meaning which in such association was meant to attach to these words, nor would I now for one moment attempt to employ them in a different sense, if I thought it would give pain to the tenderest conscience in this room. But indeed they are words of the widest bearing, and refer to feelings and attainments which lie at the very basis of all human character. We become what we are, we effect what we do, in virtue of what we love, what we believe, and what we hope. I have ventured to censure as a weakness to which those who work in pursuit of a more intimate knowledge of disease are very liable, a spirit of dilettanteism, a willingness to be contented with half results. For that weakness, the cure rests in the *Amo*. He who intensely loves will sympathise with the miseries which afflict his fellow-men, and will be ever zealous for their relief. Nor will he fail to use every opportunity of becoming familiar with the reality of those miseries, and thus warm his sympathies and increase his love. Next comes the *Credo*. Do we heartily believe in respect to the advancement of the happiness of man, in the primary value of the discovery of scientific truth? From the *Credo* to the *Spero*, in this instance the step is very easy, for they are almost phases of the same sentiment.

I have just enumerated, very briefly, a very few of the countless encouragements to hope which those conversant with the history of our profession may easily find in the records of the last half-century. Can any doubt that they are but an earnest of far greater triumphs to come? The work that is before us spreads out in a sort of threefold division. We have to apply in the best practicable manner our present knowledge for the benefit of those around us. We have to do our best to increase that knowledge, and, thirdly, we have to find the best means for transferring it to the new generation which will soon succeed to our duties. Medical practice, the advancement of the knowledge of the nature of disease, and the training of our sons—such are our three great spheres of duty. Some of us work in one, some in another, most of us to some extent in all. Let us seek to love the final object at which we aim, to believe in the means which we are employing, and to hope confidently of their results.

ON GANGRENOUS ERUPTIONS

IN

CONNECTION WITH VACCINATION AND CHICKEN-POX.

BY

JONATHAN HUTCHINSON, F.R.S.,

SENIOR SURGEON TO THE LONDON HOSPITAL AND TO THE HOSPITAL FOR
SKIN DISEASES; CONSULTING SURGEON TO
MOORFIELDS HOSPITAL.

Read October 25th, 1881.

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1882.

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Received October 7th—Read October 25th, 1881.

I HAD the honour to exhibit before the Society about two years ago, the body of an infant who had died of a severe gangrenous eruption, which had followed vaccination.

I believed that it was an example of “*Vaccinia gangrenosa*,” i.e. a vaccinia eruption which had taken on a gangrenous form. I now purpose to bring before the Society the details of the case, and also to give some account of an allied and much more common malady to which for many years past I have applied the name “*Varicella gangrenosa*.”

*Details of Case in which Death followed from a Gangrenous
Eruption after Vaccination.*

A male infant, aged about three months, and in excellent health, was vaccinated on November 11th, 1879, at a district

office. The vaccination was from arm to arm, and the vaccinifer appeared to be in perfect health. Four others were vaccinated from the same source at the same time, and nothing unusual happened. On the eighth day after the vaccination the child was brought again to the station for examination. It had four pearly vesicles on the arm, which showed nothing unusual, but its body and limbs were covered by an eruption of a peculiar character. This eruption, which had been coming out for a day or two, was described by my informant, the vaccinator, as looking like smallpox. He said that the papules were distinctly shotty, and, believing it to be a case in which variola had been contracted prior to vaccination, he instructed the mother to take the child home and on no account to bring it to the station again. Four days later he visited the child at its home and found the pustules much developed, whilst in many of them gangrene was commencing. He now advised that the attendance of the parish medical officer should be obtained. This was not done, and between this date and that of the child's death no medical man saw it. The death occurred on November 30th, twenty days after vaccination. Under the peculiar circumstances an inquest became necessary, and an opinion having been expressed that the case was one of vaccinal syphilis, I was requested by the coroner to examine the body and make a report.

The state of the child's skin after death may be realised with tolerable accuracy by the portrait (see Plate I). The vaccination spots were covered with scabs, and there was a certain amount of congestion around them, but none of them were in the least indurated.

On the scalp and face there were spots and patches ranging in size from a shot to a shilling, some of them simply congested or scabbed, but others showing a central area of gangrenous skin. On the trunk, both back and front, there were similar spots with some also much larger. All the larger ones showed a rim of deep ulceration which surrounded a central slough of black skin. From a few

the slough had separated, a deep ulcer remaining. But few of the ulcers were quite round, and in many instances the shape was irregular, as if several spots had coalesced. The eruption occurred on both sides of the body alike, but as regards the larger eschars, it was by no means arranged in exact symmetry. The back was very severely affected, and the lower part of the abdomen and upper parts of the thighs. There were some large eschars near the knees, but the lower halves of the legs and the feet, and the whole of both upper extremities below the elbows, were almost wholly free.

A post-mortem examination of the body was made for me by Dr. Barlow, who reported that the viscera were free from disease and that the child appeared to have been in a state of good nutrition.

So far as I am aware this is the first case of gangrenous eruption after vaccination which has been recorded, but that a general exanthematous rash does sometimes follow at the end of the first week after vaccination has long been well known. To this eruption the term vaccinial exanthem has been given. Several somewhat different forms of eruption have been recognised in this connection. Sometimes the rash is simply erythematous, at others papular, and in a few it closely resembles varicella. It has been described in detail by Mr. Ceely and others. I believe that the case just narrated was an example of vaccinia which became gangrenous, and it is of interest to note that the primary lesion of the skin was a hard papule with an umbilicus, and so closely resembling variola that it was taken for that disease by the surgeon who saw the case.

In a paper written some years ago, I attempted to draw attention to the fact that a peculiar form of prurigo, which is a common consequence of varicella, occurs also sometimes after vaccination, and probably in connection with the vaccinia rash. Thus it would appear that there are certain features of similarity as regards the pathological possibilities of vaccinia and varicella, and to these

I wish now to add the fact that they are both liable to become gangrenous and to end fatally.

Before I pass to the subject of varicella gangrenosa, I wish to mention another case of gangrenous vaccinia which has occurred in Dublin since my communication to this Society in November, 1879. The case occurred under the care of Mr. William Stokes of the Richmond Hospital, and I am indebted to him not only for particulars of the case, and permission to make use of them, but also for being able to bring before the Society an excellent portrait of the patient. In Mr. Stokes' case the gangrenous patches were much larger than any which occurred in mine. Those on the buttocks were several inches in length. As in my case they are irregular in shape, and evidently produced by the confluence of groups of papules. It is unfortunate that in this case there is uncertainty as to the date of vaccination and consequently as to the length of the interval before the appearance of the eruption.

Neither in this case nor in my own were the vaccination sores themselves attacked by gangrene; it was the general eruption only which was so affected. The full details of Mr. Stokes' case have been published by him in the 'Dublin Journal of Medical Science' for June, 1880.

Varicella Gangrenosa.

The earlier cases which came under my notice were mostly in the late stages of the disease, and none were so characteristic as in a later one of which the portrait is produced. In this case the patient was under the care of Dr. David Lees at the Children's Hospital, Great Ormond Street, who kindly procured me an opportunity for seeing it.

In my own cases the patients usually came under observation in the surgical practice at the London Hospital, on account of deep gangrenous ulcers in the flexures, and with a few scattered spots on the skin. It was by the history only that I was enabled to connect such cases with chicken-pox.

I am sure that I have seen five or six of this kind, but

I regret that I have not preserved notes of any in a form which would justify my producing them to this Society. I recorded in the 'Ophthalmic Hospital Reports,' vol. vi, 146, a case in which a young child after an eruption much like chicken-pox had a double irido-choroiditis, and lost both eyes. A few years later a case similar to this came under my care at Moorfields, the child being brought there on account of double iritis. The child had several scattered spots of gangrene where the vesicles had been. I suggested gangrenous varicella, but was unable to carry the proof further as there was no evidence to show that the child had been exposed to contagion.

In a lecture on some of the peculiarities of varicella, delivered many years ago, I mentioned "a form of ulcerating chicken-pox which in syphilis finds its homologue in rupia." I then quoted a passage from Trousseau, who long before had described an epidemic of chicken-pox, in which the eruption had been protracted for six weeks or two months, and had produced ulcerations like those of pemphigus.

There are in Guy's Hospital Museum excellent wax casts from two cases which I should diagnose as gangrenous varicella. They are classed under the name of rupia escharotica.¹ In the description of one of the models it is said "the earliest stage of the affection is seen in certain vesicles ranging in size from a pin's head to a split pea. Some of them are flattened and have red areola round them. Subsequently the vesicles became larger and ulcerated." It is stated in this case that the child recovered in a fortnight without having taken any medicine, a fact strongly at variance with the usual history of pemphigus. No one who has examined these models and read Dr. Fagge's descriptions, and who is acquainted with Dr. Whitley Stokes' description seventy years ago of what he called "Pemphigus gangrenosus," can doubt for a moment that the "Rupia escharotica" of Guy's and the pemphigus gangrenosus of Irish observers are one and the same malady. I hope to make it scarcely less clear that they are both of them really forms of varicella.

¹ Models 206 to 209.

Dr. Whitley Stokes, in 1807, wrote in the 'Dublin Medical and Physical Essays' a paper on "An Eruptive Disease of Children." He proposed for it the name of "Pemphigus gangrenosus" or "White blisters." He described it as very severe and frequently fatal, and said that it was well known in many parts of Ireland. It is not improbable that several different maladies contributed to his description, but that his attention had been attracted to cases of gangrenous chicken-pox is, I think, almost certain. He said that the eruption usually developed itself during perfect health, and that it occurred by preference to strong children. "One or more vesicles appear, mostly larger than the most distinct smallpox; these increase for two or three days, burst, and discharge a thin fluid and a disagreeable smell." The febrile disturbance which accompanies it he believed to be induced by the irritation of the eruption. Death usually occurred about the tenth or twelfth day, and in those who recovered relapses were common. Most of the patients were between the ages of three months and four years, but it had been observed as late as nine years. The flexures of the joints and behind the ears were frequently affected by gangrenous ulcers, and gangrenous spots were the characteristic of the disease.

Dr. Whitley Stokes distinguished the disease from chicken-pox, but he evidently came very near to the recognition of what I believe to have been its true character. In speaking of diagnosis, he wrote: "On the other hand, the swine-pox (varicella) resembles this disease in its first stage; but the fever rarely precedes the eruption in 'white blisters,' and the pustules of varicella dry quickly."

If we put aside the assumption that in all cases the vesicles of varicella dry quickly, we have no point left for differential diagnosis except the occurrence in varicella of fever before the eruption, and it is now well known that this is often, if not usually, so slight as to be easily overlooked.

Dr. Stokes mentioned that in some cases the eye

is inflamed, a fact which, as already mentioned, has fallen under my own notice. His observation as to the occurrence of spreading gangrene in the flexures is one which I can also quite confirm.

The evidence upon which I rely in support of the belief that this eruption is no other than a modified varicella is the following. It occurs to children in good health at the age at which chicken-pox is common, and it affects the parts usually the sites of that eruption. It disappears spontaneously after a short time, though on account of its local severity it is always protracted longer than ordinary chicken-pox. The eruption in most instances comes out in a single crop, all the vesicles or sores being at the same time in the same stage. In the early stage it usually closely resembles chicken-pox, and is often taken for it by the child's parents. The names which it has received in Ireland of "white blisters" and "eating hives" are very suggestive. Lastly, and I think conclusively, I have seen it in case after case in which one child in a family had this gangrenous eruption, whilst the brothers and sisters had chicken-pox in the ordinary form. Dr. John Abercrombie, of the Children's Hospital, in February, 1880, brought before the Pathological Society the dead body of a child which offered a good example of the malady, and mentioned that after the child's admission at the hospital one of its brothers developed ordinary varicella.

The constitutional disturbance which attends this form of eruption frequently runs high. Dr. Whitley Stokes spoke of the disease as being often fatal, and I have myself known of several deaths. The cure, however, when healing commences, is usually rapid; all the sores, as a rule, cicatrising simultaneously.

In explanation of such exceptional severity of a disease, so common and usually so insignificant, it is impossible to do more than fall back on the suggestion of individual idiosyncrasy. The severe form does not happen to delicate children more frequently than to the robust, indeed, in

several instances in which three or four brothers and sisters had varicella at the same time, whilst in only one did the eruption become gangrenous, the subject of this latter form was by no means in more feeble health than the others. I have never seen more than one child in the same family affected by the gangrenous form.

In conclusion, I should attempt a summary of the statements which I wish to offer for the consideration of this Society.

I have tried to prove that it is possible for the eruption of varicella in isolated cases and in connection with idiosyncrasy on the part of the patient, to assume a very severe type, becoming bullous, petechial, or even gangrenous. In these gangrenous forms there is much constitutional disturbance, and death may result. Now and then very dangerous forms of iritis or of panophthalmitis are witnessed. Next to the assertion that there is such a malady as gangrenous varicella and in part based upon it, comes a second proposition to the effect that the *vaccinia* eruption may also assume the same type and with similar danger to life. That there does occasionally occur after vaccination a general eruption all observers admit. I have adduced only two examples of the gangrenous form of this eruption.

Respecting my own case I do not think that there can be any reasonable doubt. The child was in excellent health, was successfully vaccinated, and was affected seven days afterwards by a general eruption which was taken for small-pox. This eruption became gangrenous, and the child died of exhaustion on the twenty-first day. The eruption may have been variola, varicella, or vaccinia; that it was one of these three is almost certain. In Mr. Stokes' case the facts are similar, with the exception that it is impossible to assign the period of incubation correctly. That the vaccination sores themselves remained in both cases free from gangrene has been alleged by some critics of Mr. Stokes' case as a reason for doubting whether there was any connection between them and the eruption. But

such doubt is, I submit, not reasonable. It is the exanthem, and not the site of inoculation, which is attacked by gangrene, and surely there is no *à priori* reason for expecting that the latter should suffer.

In syphilis a very severe form of rupial eruption may follow in cases in which the original chancre was a mild one. In my own case the death of the patient deprived us of an opportunity of observing the healing of the sores, but it is to be noted that they had all at the same time advanced to pretty much the same stage. In Mr. Stokes' case the healing of the sores simultaneously gave support to the belief that they were of the nature of an exanthem, and not in any sense accidental results.

APPENDIX.

The following are descriptions of some cases not mentioned in the paper and of drawings which were exhibited at the meeting.

1. A portrait lent me by Dr. Barlow, from the collection in the Children's Hospital. The infant died with an eruption consisting of gangrenous patches exactly like those shown in the previous and following portraits. No history as to varicella has been preserved.

2. A portrait lent me by Mr. Waren Tay, showing the character of the eruption in the chest of a young child. Two round sores are seen, each with a central eschar of gangrene, as if punched out. In the first instance the child presented an eruption of varicella, which was seen and diagnosed by Dr. Sansom, of the North Eastern Hospital for Children. A considerable number of the spots subsequently became gangrenous, and passed into the stage shown in the portrait. Mr. Tay was kind enough to give me an opportunity of seeing the child just before the portrait was taken. I am not able to state with positiveness any facts as to family history, but I believe that varicella had occurred in the family.

3. A portrait lent me by Dr. Barlow from the collection in the Children's Hospital, showing varicella as an ulcerating, bullous eruption, not absolutely gangrenous. This portrait is of great interest as illustrating a lesser degree of severity in the inflammatory process. It shows well how large irregular sores have been formed by the coalescence of groups of bullæ. The shapes of these sores are exactly like those seen in gangrenous cases. The severity of the eruption on the trunk and head, and the comparative exemption of the extremities, is also well seen. This probably well illustrates the eruption described by Trousseau as pemphigoid varicella.

4. Portrait of an infant aged eight months, named Sawyer, who came under my own observation in August, 1880, in the condition shown in the sketch. There was a history of gangrenous varicella six weeks previously. The portrait was taken in order to show the kind of scars which are left by this eruption, and to show that they might easily be mistaken for those of syphilitic disease.

The following are some particulars respecting other cases not mentioned in my paper.

Dr. David Lees gives me the particulars of a case of the child, George Watson, aged fourteen months, who was under his care as an out-patient in January, 1880. Varicella had appeared twelve days before, and four other children in the same house had it. In this child the spots had developed into deep sores, and in some on the parietal region there were black eschars appearing. One on the left cheek was so deep as almost to involve the mucous membrane. A subcutaneous abscess was formed in the neck.

Dr. Lees also gives me the notes of a child aged a year and a half, named Sarah Ann Reed, who in December, 1879, had a vesicular eruption, from which resulted a number of punched-out sores on the lower part of the back.

Also a third case, in which a female child, aged nine months, under Dr. Dickinson's care, had undoubted

varicella, had subsequently deep ulcers on the upper and inner parts of the thighs. There were no actual sloughs, but foul ulcers as large as sixpences resulted.

DESCRIPTION OF PLATE I.

(Gangrenous Eruptions in connection with Chicken-pox and Vaccination. JONATHAN HUTCHINSON, F.R.S.).

A portrait of the back of the child whose body was shown to the Society in November, 1879, and in whom eruption resembling variola appeared on the sixth day after vaccination. The drawing was taken after death, which had occurred from exhaustion on the 20th day. It will be seen that the vaccination spots, four in number, are somewhat inflamed, and show pus scabs, but they are not gangrenous. The trunk and upper parts of limbs are covered with gangrenous patches.



INTRODUCTORY ADDRESS

AT THE

OPENING MEETING OF THE SESSION 1883-4,

October 11th, 1883.

By JONATHAN HUTCHINSON, F.R.S., President.

GENTLEMEN,—We begin to-day the fourth session of the Ophthalmological Society of Great Britain. In the first place I must thank you heartily for the honour you have done me in electing me your second President. Appreciating this honour most highly, I yet accept it with much misgiving, more especially when I remember who has been my predecessor.

In Mr. Bowman you have enjoyed the services of a President of unequalled fitness and ability—of one, indeed, who had already conferred inestimable benefits on ophthalmic science. His acceptance of the office of President at once secured the success of our infant Society, and in his recent resignation of it we have sustained a heavy loss—one which, I am well assured, it will be quite out of my power, in any degree, to make you forget. The chief satisfaction which I have, in venturing to accept your invitation to succeed him, is the knowledge that he still takes the warmest interest in our affairs, and that I shall always have the advantage of his advice and help.

It will be my duty to mention to you directly, facts which will prove that, although Mr. Bowman has resigned the nominal presidency, he still occupies in relation to us an almost paternal position. Long may he live to do so!

We meet this evening, as you will have observed, in

rooms which have been made much more commodious since our last session, and in connection with this subject I have to give you some important items of information. The first is, that the Medical Society of London, whose tenants we are, on entering upon their greatly improved premises, felt themselves obliged very materially to increase our rent. There was nothing in the least unreasonable in this; indeed, our landlords have throughout acted towards us in a liberal spirit. The proposed increase was, however, in respect to our finances, a very heavy one, and as the Council was desirous to collect a library, and to form a museum of instruments and appliances—both objects demanding outlay,—we found ourselves for a time in a position of great difficulty. It is almost certain that the Society could not have afforded to continue in these rooms and develop itself in the proposed directions had it not been helped by an act of scientific beneficence not often surpassed.

Having acquainted himself with the facts, and noting our position, our ex-President made an offer to the Council to himself undertake the cost of purchase of all fittings necessary for the museum and library, and further, to make a gift to the Society annually, for twenty years, of the sum of £50, in order to defray the expenses of rent of rooms. Need I say that the Council on your behalf thankfully accepted Mr. Bowman's munificent offer, and I have now the pleasure of informing you that we are, in all probability, rent-free for twenty years, and shall be able to devote the whole of our income from subscriptions to the publication of our annual volume. I am sure that you will receive this most liberal gift of your past President as one in the highest degree worthy not only of praise in the present, but of imitation in the future. The pecuniary advantages which it confers upon us are solid, extensive, and durable; but, warmly as we appreciate them, I believe I may say that those who have been most closely associated with the early years of our Society derive yet greater pleasure from the fact, that one so

competent to judge, should in so emphatic a manner have crowned their efforts with his approbation.

I have next to allude to a remarkable coincidence. Mr. Bowman's offer was made only about a month ago, and quite unexpectedly to us all, whereas for at least three months before this, and wholly unknown to him, the Council had had under consideration a proposal to recognise his pre-eminent position in respect to ophthalmology in Britain, and the invaluable services which he had already rendered to our Society. It was from Dr. Gowers that the suggestion had first come that we should found a lectureship to be known as the Bowman Lecture, but it was no sooner mentioned than it was received with unanimous approbation.

I am anxious, for reasons that will be self-evident, to make it clear that the Council's endeavour in this slight manner to do honour to Mr. Bowman, and his liberal endowment of the Society, had no connection one with the other, although the two projects ripened about the same time. Dr. Gowers' proposal has been several times discussed in our Council meetings, and should it meet with your approbation, as I feel sure that it will, the lecture in question will be founded forthwith. Without wishing unduly to bind the executive in future years, the present proposal is that a Bowman Lecturer shall be appointed each year, and invited to prepare for us a critical summary of the best extant information upon some special subject to be selected by the Council, or, if not selected, approved by it.

This lecture will probably be an annual one, and will be delivered at a meeting specially appointed for that purpose. We hope in it not only to permanently associate with our Society the name of a great man, but also to contribute each year something real towards that "advancement of knowledge for the good of man's estate," which has been Mr. Bowman's lifelong aim.

With this statement I end, gentlemen, the announcements which it has been my most pleasing duty to make

to you, but I purpose before sitting down to trespass upon your attention with a few further remarks on our general position and the possible scope of our future work.

I think that we may now fairly congratulate ourselves that the organisation of our Society, if we regard it simply as providing means for the furtherance of research in ophthalmology, is well-nigh perfect. We shall have regular meetings in commodious and central rooms, at which any subject which is brought forward will be certain to receive the attentive criticism of an audience, than which none exists better qualified for the task. We invite in the freest possible manner the production of all new facts, opinions, and suggestions, whether relating to extended series of observations or isolated cases. All that concerns the eye, whether in health or disease, concerns us, and we shall be thankful alike for the single case and the elaborate paper. Nor is there, I am happy to say, any spirit of exclusiveness as regards membership with us. We shall willingly accept the help of all who take an interest in our pursuits. Those who had the largest share in the formation of this Society were careful that it should have a wide basis, and, thanks to their foresight, it has now the good fortune to include amongst its members many, physicians, surgeons, and others engaged in general practice, who are not, and never have been, in any sense, specialists.

To say nothing of the original contributions which we have had from some of these, their help in our debates and their services on our committees have been, and will be in the future, simply invaluable. It is true that we have not yet a library of reference, or a museum. But the first of these desiderata will, I doubt not, soon be supplied, and the other will be put in course of formation to such extent as may suitably come within our lines of work. We shall probably never attempt the formation of a pathological collection, since we have no convenience for its preparation or its display, and there exists, besides, at other institutions, ample provision in this direction.

We do, however, contemplate the formation of a collection of instruments and appliances, and to this object Mr. Bowman's endowment will, as I have said, be in part devoted. Probably also we shall make gradually a collection of drawings and other forms of graphic illustration. These can be easily classified and stored for reference in the drawers of our library. Should it occur to any of our more wealthy friends to emulate Mr. Bowman's noble example, I cannot, for my own part, think of any object to which a second endowment could be more usefully devoted than to the formation of such a collection.

Morbid conditions of the eye, whether external or revealed by the ophthalmoscope, lend themselves with peculiar facility to the artist's skill. If we had the funds I would suggest that, under the auspices of a committee, we should copy, collect, and classify, from all available sources, private and public, published or otherwise, all such illustrations of eye disease as are passably good in execution, and duly authenticated and described. With but few exceptions I would leave aside all in which the history of the individual case is omitted. If this scheme were completed we should find, if I am not mistaken, that we were in possession of a sort of clinical museum which would prove of very great use alike to students and to all engaged in original research. I certainly count this object as chief among the desiderata for which adequate provision has not yet been made.

Hitherto I have been speaking of our arrangements and organisation as a Society for the improvement of knowledge in our special branch. To those who, with me, believe that it would be difficult, in any material degree, to alter these arrangements for the better, it is, I may repeat, a source of great satisfaction to know that they have received the emphatic *imprimatur* of our first President, than whom there is no man living so well qualified to judge.

The improvement of ophthalmic knowledge is unquestionably our first, and by far our principal duty. I

cannot but think, however, that it is possible that in the future such societies as ours may find another kind of work open to them, which is only second in importance.

I allude to the systematic and strenuous endeavour to diffuse rapidly amongst the profession at large, for the prompt benefit of our patients universally, all items of new knowledge which may have been obtained.

There are many directions in which thoughtful help might be given towards this end. We may, in the first place, endeavour to induce as many as possible to join us, and attend our demonstrations and discussions and receive our volumes. We shall not, however, in this way reach many excepting London residents.

It is perhaps possible that something might be done to make some of our meetings, and the reports of them which appear in the journals, more valuable to the bulk of the profession, by becoming less definitely special than they now are. We might, for instance, bring forward for discussion, occasionally, the commoner forms of eye disease—such as are scarcely likely to be often made the themes of original communications. Not only would this help others, but it is very desirable for our own good that we should occasionally make recapitulation in public of our knowledge of common things, and thus ascertain how far our opinions have advanced towards unanimity.

There is another branch of the same topic on which I incline, if you will permit me, to enter into a little more detail, since it offers possibly a sphere for much useful work in the future on the part of societies like ours. It is one, indeed, to which perhaps this Society in particular is more specially called than any other. I refer to the promotion of what may be named *every-day therapeutics*. It is obviously quite possible that the knowledge of diseases of the eye might be cultivated by a few up to a point of very high excellence, and with great finish of detail, and yet remain a possession of the specialist, and benefit but little the family practitioner, and the public his patients. In some degree this state of things is un-

avoidable, and in some departments of our practice we cannot hope to ever escape it. Still, however, it will be admitted by all to be a matter of regret. So far as we can do it, it is our duty to make such knowledge popular—to diffuse it over an area the widest that we can obtain. A practical knowledge of astigmatism is not to be expected from a general practitioner; possibly not even from all who are engaged in the treatment of eye diseases as a specialty. The attempt to use the ophthalmoscope for purposes of diagnosis, although quite possible to a large section of the younger part of the profession, enjoying constant opportunities and fresh from hospital training, would probably, to by far the greater part, prove to be a source of error rather than a help.

Skill in the diagnosis and, as a necessary consequence, in the treatment of a not inconsiderable group of rare diseases of the eye, must always, despite any development of education which it is reasonable to hope for, and any artificial aid which can possibly be given, remain the possession of the specialist only. But it is otherwise in respect to a majority. Almost all the examples of the commoner forms of eye disease come under the care, in the first instance and often throughout, of those who are not specialists, and have perhaps never even had any training in an ophthalmic hospital. Circumstances over which no one has any control render this inevitable. Whether or not the surgeons concerned desire it, they must perforce take charge of "eye cases" as well as of others. It is in reference to practitioners so placed that I would suggest that our Society has possibly a duty to perform. If I trouble you with a few examples, I shall probably be best able to convey my meaning.

Concerning the treatment of syphilitic iritis, there is probably but little hesitation or difference of opinion amongst specialists, and perhaps I could hardly mention another disease respecting which the opinions of specialists are more widely known and accepted. That atropine should be used from the first, frequently, freely, and in

strong solution, and that mercury and iodide of potassium are very useful and ought always to be given, but in no degree compare in importance with mydriatics, I take to be the acknowledged canon. It would be easy to prepare an explicit *schema* for the treatment of this disease, giving the exact strength of the atropine, the frequency of its application, the precise dose of the mercurial, and suggesting a few of the more important means which help success, such as a purgative, leeches to the temples, and low diet. This might be done in ten lines, and so printed in a visiting-list or pocket-book that it should be readily accessible to all. It would be better that such a schema should be propounded under the auspices of a Society than that it should come from an individual. In many parallel instances, the discussion and examination which such *schemata* of treatment would receive at the hands of our Society would, no doubt, be of great use in perfecting them, as well as in adding to their authority.

I do not doubt that there are, at the present moment, whilst I am speaking to you, in the homes, the schools, the workhouses, and the hospitals of England, some thousands of children who are suffering from ulcerations on the cornea, attended with intolerance of light, causing the patient great distress and annoyance through many months, and destined often to leave disfiguring and incapacitating scars. If my own experience may be trusted, I believe that three fourths of these would be almost well in the course of a fortnight under the use of a very weak yellow oxide ointment. Many of them, no doubt, are getting it, but a considerable majority probably are not; for this plan of treatment is not yet universally acknowledged among specialists, and certainly not very widely known in the profession.

If this Society could, after an examination of the subject, determine upon the recommendation of an explicit formula which would be likely to result in the prompt cure of these very troublesome cases, it would confer an immense boon upon the public. Such a formula, so

recommended, would be copied into every medical journal and into every manual. It would be reprinted over and over again, and would become the property of the whole profession.

Is it not somewhat humiliating to reflect that if a quack were to bring out a very weak Pagenstecher's ointment, give it a telling name, and push it into notice as a specific for chronic inflammations of the eye, he would be a public benefactor? No doubt it would often be used in error, but it would even then do little or no harm, and I have not the least doubt that the balance of gain would enormously preponderate. My own experience has been, that since I knew the virtues of this ointment I have been able to abandon almost entirely the use of blisters, setons, and like painful measures, and to effect the cure in a tenth of the time. I have reason to think that a large majority of ophthalmic specialists have had a like experience. Yet we hesitate to come boldly before the general profession and announce loudly an important item of progress. We fear to boast, we dread to impair the scientific spirit by the formation prematurely of general rules; and, seeking to quiet our consciences by reminding ourselves that after all the thing is no secret, we do nothing further in the matter. Our reticence is a loss to the nation, it is an injury to hundreds and to thousands whom the benefits of modern ophthalmological science might reach if we would only consent to throw away our fastidiousness. Is it not a frequent failing among the more scientific part of our profession to become superfine? We dread the spirit of the charlatan and the self-seeker so much, that we come, like David when in presence of the sinner, to "hold our peace *even from good.*" In the individual, scrupulous care in these respects is most meritorious; nothing is less to be desired than that those who believe themselves to have made therapeutic discoveries should deem it their duty to proclaim them ostentatiously. Let them be brought forward in the first instance quietly, and under the cognisance only of those skilled to judge of them.

But the fact that it is meritorious in individuals to abstain from pushing their favorite remedies, only throws the duty, to which I have been alluding, the more definitely upon public bodies like ourselves. No one could impugn our motives or doubt our sincerity, and our verdicts would be received not certainly as final, but as entitled, at any rate, to a temporary acceptance.

Let no one suspect me of wishing to stereotype knowledge or to damp the ardour of any skilled person in the endeavour yet further to improve our therapeutic resources. There is no fear in that direction; and what I am concerned to assert is this, that nine out of ten of the practising part of the profession would most thankfully receive from this Society detailed *schemata* for the treatment of various typical forms of eye disease. Let me further add—without, I hope, hurting anyone's feeling—that I am sure that the use of them would tend immensely to the benefit of their patients as compared with the extemporised prescriptions now employed. It is not in the power even of the most laborious of those engaged in family practice, to keep their minds well stored with details respecting the management of diseases which, although very common with us, are rarities to them.

I might easily mention a number of special types and forms of eye disease—purulent ophthalmia, rheumatic iritis, episcleritis, catarrhal ophthalmia, glaucoma, and the like—for which definite schemes of treatment could easily be laid down. It will, I have no doubt, be objected, that, after all, successful treatment depends upon the correctness of the diagnosis. This statement is almost as obvious as was the famous injunction to “first catch your hare.” It is no reason that because diagnosis is difficult, therapeutics should be left in a muddle also.

I might urge further that I believe, working on the same lines, this Society might do much to put the diagnosis of eye diseases more easily within the reach of British practitioners in general.

There is no one present who has not been pained over

and over again by having to treat cases of glaucoma which were brought to him too late. In spite of all that has been done by specialists, and in spite of the fame which iridectomy cures have obtained, it is still the fact that a large proportion of cases of acute glaucoma are unrecognised during the first fortnight by those under whose observation the patients come. Practitioners of the most scrupulous care, of wide general information, and the most conscientious regard for their patient's good, are yet very commonly misled by the acute congestion and severe constitutional symptoms which often attend the early stages of this disease.

It was my fortune, some years ago, to operate upon three cases of this kind in one week, in all of which the proper time for interference had been allowed to pass by, on account of the patients' severe general illness.

In one instance I became acquainted with the facts of a case in which a benevolent country surgeon, aided by two or three friends, was himself maintaining a lady who had lost her sight, and consequently her occupation, from double acute glaucoma. He had himself attended her from the beginning, and when I gently hinted at the possibility—to me, a practical certainty—that iridectomy at the proper time would have saved the lady's sight for the rest of her life, he promptly replied “that the eyes were so much inflamed in the first instance, and the patient so ill, that he was quite sure I should never have thought of operating.” I said no more, for it would have been cruel to tell him that these were the very symptoms which denoted the necessity for an operation.

Some years ago, in the early days of the keratome, I felt so strongly on this subject that I had some thoughts of engaging a full page in the ‘Lancet’ for a big red-lettered anonymous advertisement, so staring that all must read it, stating in a dozen words the symptoms and inevitable result of glaucoma, together with the certainty of its cure by operation.

And now, looking back upon such impulses of enthu-

siasm, I do deliberately declare my conviction that a Society like our own would have been more than justified in taking such a step. At that time acute glaucoma probably had, on British soil alone, its daily victim, whom it left in irrevocable blindness. In the present day the number has been greatly diminished, but it is still, no doubt, very considerable. Our confidence in the remedy which we then hailed has remained unshaken; and it is most certainly a very melancholy thought, that there are thousands now living without sight who might have saved it very easily had there existed any efficient means for the rapid diffusion of the new knowledge.

I must not trespass further upon your patience in this matter. Briefly, what I desire to urge is this, that we ought not to be content with doing our utmost to make knowledge perfect, and to secure its application in our own immediate spheres of action, but that it is well worth a thought whether Societies like our own have not duties to perform in respect to its diffusion. I will not for a moment doubt that a subject so important will receive from you such attention as your judgments may deem it entitled to.

Is it too much to hope that something of the nature of a compendium of ophthalmic therapeutics may sometime be prepared, which shall bear the authority of a Society's consensus? Such a code should of course be destined to modification from time to time, but it would probably from the first be a great advance upon the statements of any individual, both in explicitness, in brevity, and in the amount of practical experience which it would summarise.

Should the Society see its way in the future to any action in this matter, much collateral advantage might be expected by the more detailed attention to therapeutics which would be given by the committees appointed to report.

Had time permitted, I might have ventured to bring before you a few other suggestions as to work which the

Society might undertake collectively—such, for instance, as a systematic examination of symptoms with the object of defining and describing them more accurately ; of preparing detailed lists of the more rare types and forms of disease, and giving to each its concise description ; and possibly, after this were done, of preparing nosological lists which might assist the labours of hospital registrars.

I have occupied, however, already much more time than I had intended, and must not detain you any longer from the proper work of our meeting.

Case of proptosis, first of one and then of the other eye, in association with enlargement of various glands.

By JONATHAN HUTCHINSON, F.R.S.

MR. S—, a Hindoo barrister, came over to England in September, 1882. I saw him in consultation with his brother, who was a surgeon, on the day after he landed. His right eye had been lost by inflammation after an operation for the removal of an orbital tumour, and his left eye was now in a condition of extreme proptosis. The lower lid was everted, and the whole of its mucous membrane exposed. The prominence of the eyeball and the eversion of the lid were very much greater than they are shown in the photographs, as these were not taken until about two months after the commencement of treatment.

Mr. S— believed that the condition of his left eye was now almost exactly similar to that of the right at the time the operation was performed. It is to be noticed that the proptosis was decidedly downwards. The movements of the eye were not much interfered with, but there was much conjunctival congestion and chemosis. The edge of the lacrimal gland, greatly enlarged and very firm, could easily be detected on a level with the upper margin of the orbit. On careful examination I could not feel certain of the existence of any definite tumour-growth in other parts of the orbit.

The upper eyelid hung rather loosely, but it was of course quite impossible to close the lids. There was a general puffiness of the whole of the face, especially in the parotid region and under the jaw. The subcutaneous

cellular tissue and fat being abundant definite examination was rendered difficult.

Mr. S— brought with him an excellent narrative of his case, written out by Mr. Cayley, of Calcutta, who had attended him there, and by whom the operation had been performed. It appeared that Mr. S— had always had full eyes, and that about three years ago it was for the first time noticed that his right eye was rather more prominent than the other. There was, however, nothing that was inconvenient or unsightly until about two years later, when the prominence had very greatly increased, and a firm tumour could distinctly be felt in the region of the lacrimal gland. Subsequently another growth was recognised in the lower part of the orbit. There was little or no pain, and Mr. S— was in his usual health. By degrees the eyeball was so much pushed out that the lids would not cover it. Mr. Cayley's notes state that the cornea had begun to look steamy, and the sight was somewhat affected. In April, 1882, an operation was performed, and the external canthus having been freely divided, the lids were dissected up and down, and first the lacrimal gland removed and next a firm lobulated mass, which occupied the outer and lower part of the orbit and adhered firmly to the periosteum. As far as could be ascertained, the whole mass was got away, and without injuring the eye or its muscles.

For a few days after the operation sight remained good, but eventually suppurative inflammation of the orbit ensued, the eyeball was again pushed out, and the cornea sloughed. After this the remains of the eyeball collapsed and receded, no fresh growth in the orbit taking place. The tumours removed were examined by Dr. MacConnell, the pathologist of the Calcutta Medical College, who described the one as glandular and developed from the lacrimal gland, the other as composed of fibro-adipose tissue, the fibrous elements preponderating; ("delicate, nucleated, fibro-elastic filaments").

Such was the history of the eye which had been first

affected and which was now lost. It is to be especially noted, as bearing upon the nature of the new growth, that there had been no recurrence, the condition of the parts in the orbit being much as is usual after suppuration of the eyeball from any other cause. At the time that the operation was performed there was no reason to suspect anything amiss with the left eye, but within a week Mr. S— was alarmed by finding that it was taking on exactly the conditions which had been observed in the first stages of the right. It became prominent for a time and then receded, and then became prominent again. He now determined to come over to England for advice, and left Calcutta on July 22nd. During the voyage the proptosis very greatly increased.

The condition of things at the time that he landed I have already described. It was sufficiently alarming, and Mr. S— was in great distress, regarding the loss of his remaining eye as almost inevitable. On the most careful examination I could detect nothing in his general health nor elicit any facts in his personal or family history which threw any light on the nature of the disease. He had worked hard in his profession, but had always enjoyed fairly good health. The proptosis, although attended by some difficulty in breathing, had not been associated with headache, nor as yet with any material diminution of sight.

As the result of the operation in the other orbit had not been encouraging, I decided to try, for a time at any rate, other measures. Mr. S— was admitted into Fitzroy House, a hospital home, and was treated by the sedulous application of ice over the forehead, the eye itself, and to the back of the neck. He also took six grain doses of iodide of potassium. Within a week a very considerable improvement had taken place; the eyeball was less prominent, the swelling of the conjunctiva less, and the lacrimal gland not so easily felt. The same measures of treatment were persevered with for about a month, when the recession of the eyeball was such that he could

close the lids. It was at this stage that the photograph was taken.

There was still, however, a roll of everted mucous membrane visible below. At this time Mr. S— was allowed to go out, and the use of the ice was much interrupted ; for a week also he omitted the iodide. A relapse took place, and the border of the lacrimal gland again came prominently forward. I now suggested a consultation with Mr. Bowman and Mr. Nettleship, and this took place in the early part of November. It resulted in an increase of the dose of the iodide and the addition of small doses of mercury.

It should have been stated that before this I had discovered that the edges of the parotid gland on each side could be distinctly felt, projecting as a firm lobulated mass forwards on the masseter. The edge of these glands, although not quite so hard, was to the touch remarkably like that of the lacrimal gland. There was also some enlargement of the lymphatic glands on both sides of the neck. The result of the more vigorous treatment was as satisfactory as it had been in the first instance, and my note on December 6th states that the eye had receded so that he could again quite close the lids, that the lacrimal gland could be discovered only by deep pressure, and that both the parotid and the lymphatic glands were very much reduced in size. Mr. S— at this time appeared quite well and was accustomed to take much exercise.

A month later he returned to Calcutta. The eyeball at this time had receded almost to its natural position, but there was still a narrow rim of everted mucous membrane visible between it and the lower lid. He was to continue the iodide and increase the dose if threatened with relapse.

In attempting to investigate the nature of this remarkable case, we must remember that amongst the conditions which were demonstrable was the enlargement of glands of three different functions, a salivary gland (the parotid), the lacrimal gland, and the lymphatics of the neck. In

each instance the enlargement was firm and fleshy, quite painless, and without tendency to inflammation. The increase in size of the lacrimal gland, although coincident with the proptosis, certainly did not cause it. The gland overhung the eyeball and was moveable on it. We are driven therefore to the belief that there must have been some swelling of the fibrous or fatty contents of the orbit which caused the prominence of the globe. Whatever it was it appeared to be capable of spontaneous diminution and prone to relapse, and to be, as well as the enlargement of the several gland structures, definitely under the influence of the iodide of potassium.

Remembering the degree of recovery which took place and the fact that there has been no fresh growth in the right orbit, it is not possible to entertain the suspicion of malignant disease. My impression is that the case should be placed in the same group with certain rare examples of the symmetrical formation of ill-defined but more or less lobulated masses of fibro-fatty tissue in the region of the neck. This affection was, I believe, first well described by Brodie. I have seen several examples of it, and Mr. Morrant Baker has reported a series of cases in the 'Transactions of the Clinical Society,' and has very carefully investigated its nature. In several cases portions of these tumours have been excised and demonstrated to consist of fibrous and fatty tissue. But in at least one case under my own care there appeared reason to suspect that the case was complicated by adenoma, death ensuing with symptoms of intra-thoracic disease. I have never as yet in any of these cases observed the symptom of proptosis, nor witnessed enlargement of the lacrimal gland, but in a case which was sent to me recently by Mr. George White, of Hackney, the parotids were enlarged in exactly the same way as has just been described in the case of Mr. S—.

In this instance, the patient, a man *æt.* 40, had huge symmetrical masses on the back of his neck and under his jaw, whilst in each forearm near the elbow there were

several of the common subcutaneous fibro-fatty tumours. I show a photograph of this patient.

I excised a portion of one tumour and proved that it consisted of fat. It is to be clearly recognised that in these cases the condition is one not of new growth, but of hypertrophic development. The fatty masses are continuous with the subcutaneous fat. They are, however, remarkably local and are not coincident with any marked tendency to general obesity. In the case first mentioned they were associated with isolated fatty tumours in the forearms.

It seems probable that we ought to widen our views of this group of affections and not too much restrict conception of its features to the typical and more common cases described by Brodie and Mr. Morratt Baker. In these the patients are almost always men, and the fatty outgrowths occur at the back of the neck. In women, if I mistake not, there is a parallel affection in which the fat accumulates not at the nape, but deep in the root of the neck. The cases which I have cited prove that in some instances there is with the tendency to local fat hypertrophy a liability to increase in size of glands. Perhaps on closer examination we may find that this conjunction is the rule instead of the exception. Very probably the apparent increase in the size of the glands is due rather to overgrowth of interlobular fat and cellular tissue than to increase of gland elements. This would explain why we find glands of very different functions simultaneously affected. There may be cases—and I think I have seen some—in which general hypertrophy of glands of the same nature occurred without any form of fatty outgrowth; for example, symmetrical hypertrophy of the parotids. The physical cause of the proptosis in Graves' disease is not well understood, and in many cases it appears to be in part at least hypertrophy of fat. This singular malady may possibly be a member of the same family group.

It is to be noted that some of the cases of the Morratt Baker type are accompanied by very marked disturbance

of nerve functions. The man whose case I have mentioned was excitable to the verge of insanity. It is to be noted further that these fatty outgrowths are liable to remarkable alterations in size in connection with the state of health and mode of life of the patient. Mr. Baker has observed that they usually happen to be heavy beer drinkers and are benefited by abstinence. I can corroborate this observation, and may add that I have known definite reduction of size from change of air from London to the country.

I fear it may be thought that I have entered upon a disquisition which is surgical rather than ophthalmic. It must be remembered, however, that my aim has been to discover the nature and probable cure of a malady which is special so far that it leads to destruction of the eyes, but which probably in all its relationships outsteps the domain of the ophthalmologist.

(July 3rd, 1884.)

ON THE RELATION
OF
CERTAIN DISEASES OF THE EYE TO GOUT.

BEING

THE BOWMAN LECTURE,

Delivered Thursday, November 13th, 1884,

By JONATHAN HUTCHINSON, F.R.S.

GENTLEMEN,—At the request of your Council I have the honour this evening to deliver the first Bowman Lecture. This lecture has been instituted with the design of gratefully commemorating the services of Sir William Bowman to science, and more particularly those which he has rendered to this Society in his capacity as its first President.

It is intended that the lecturer shall on each occasion undertake the investigation of some special subject in connection with Ophthalmology.

I do not think that any more appropriate method could have been devised by which to pay respect to one of our very foremost workers. For myself, I can only say that I feel deeply the responsibility of the yet very agreeable duty which has devolved upon me, and could heartily wish that your Council had selected some one more capable of doing it justice. Having made this personal apology, I feel, however, that I shall best honour the name of Bowman by trying to imitate the example of his life, and

proceeding at once, to the best of my ability, to deal with the subject in hand.

I take for the topic of my lecture the relations which exist between certain Diseases of the Eye and Gout. We shall have to classify several different forms of disease—for the most part, but not exclusively, inflammations of the iris—to describe their peculiarities and to examine the evidence which is forthcoming as to their real connection with gout. The inquiry is an important one, for a gout taint is of extremely common occurrence in a large proportion of the British population. The investigation is also in many of its ramifications one of great intricacy and difficulty. Certain facts we shall, I trust, be able to establish with clearness, but respecting many others we shall not be able to get further than the stage of more or less confident inference. I had at one time intended to bring before you a good deal in the form of attempted proof—that is, of statistical or tabular statements of fact. I have found, however, in the preparation of the lecture that the time at my disposal will not permit of more than a brief sketch of the whole subject, and I shall therefore, on compulsion, spare you much of the detail. It is, however, only just, both to you and to myself, to state that my arguments and assertions this evening will be based upon the careful examination of a lengthy series of recorded cases. A large part of these will be found given in detail in vols. vii and viii of the Ophthalmic Hospital Reports, and the remainder I have in type before me, although not yet published.

DEFINITIONS OF GOUT AND RHEUMATISM.

Before we proceed to inquire whether gout may be deemed to be the cause in some instances of such diseases as hæmorrhagic retinitis, recurring iritis, glaucoma, and others, it may be well to state that I wish to keep a clear distinction between gout and rheumatism. This distinction

is in practice often very difficult and sometimes quite impossible to draw, but for all purposes of accurate clinical pathology we must insist upon its existence. By gout I shall mean all states of health which are, whether directly or remotely, connected with the accumulation of lithate of soda in the blood, as the result of overfeeding or defective assimilation.

Under the laws of inheritance of morbid tendency it may easily be the fact that a person may feel the results of this condition in his parents, who may yet never himself exhibit it. We may have a form of inherited gout which is not associated in the patient with either lithiasis or lithæmia. Hence, as I shall have to show presently, some very peculiar forms of morbid action.

Thus it may perhaps be conveniently laid down that there are two fundamental conceptions of what gout is. They are not antagonistic, but rather in mutual co-existence. In practice the two methods of influence constantly modify each other.

What I may call the primary conception of gout regards it as due to a state of the blood induced in part by errors in feeding, and in part by defects in assimilation and depuration. It has regard to the food, the digestive power, the habits as regards exercise, and the integrity of the kidneys. In this form of dietetic, or humoral, gout the stage of lithæmia is essential, and the final product of deposit of urates in the tissues almost equally so. We must, however, seek a wider vision than this. As an introductory stage let us imagine the condition of a man who many years ago was subjected for a long period to the ordinary causes of primitive or humoral gout, but who for a considerable series of years has carefully avoided them. In such a person it is easy to see that although his blood may have been long kept free from excess of urates, yet, as the consequence of conditions of long duration in the past, his tissues may have received modification, and may be prone still to suffer in a peculiar manner when exposed to the ordinary exciting causes of

disease. In particular they may be liable to grow old on a peculiar type. Not only are his tissues in general modified, but those of his nervous and vascular system have probably been especially influenced. Hence the power of control over any process of inflammation, by whatever cause it may have been initiated, is peculiar and defective. If such a man sprains a joint it will not recover in the way it would have done had he never had gout, yet the joint may nevertheless be very far from assuming a typical gouty state. There may be no tendency whatever to the deposit of urates. Now let us take another step in the attempt to give reality to our conceptions of the hidden influences under which the every-day phenomena about us are produced. Let us imagine that such a person as we have supposed, one who has suffered severely, and through many years, from humoral gout, becomes a parent. His offspring will inherit his tissues, his tendencies as regards digestion, kidney excretion, skin elimination, and all the rest. These will, no doubt, give a greatly increased proclivity to common typical gout should the causes of it come into play. They may, however, do much more than this. They may render their subject liable, quite independently of exciting causes, or with but little help from them, to forms of inflammation or of degraded nutrition, which are not unequivocal gout, but which are yet the direct consequence of it in predecessors. The modification which is here inherited is, I repeat, of the tissues and not of the blood, though it is obviously very easy that the two may complicate each other. The subject of tissue modification from inherited gout may easily become the victim of humoral gout as well.

INHERITED GOUT.

It is clear from this argument that we must be prepared to see inherited gout assume very different forms from those in which we are so well accustomed to recognise the

primitive type. Permit me now to suggest that it is the clinical, or rather the social, fact that a not inconsiderable proportion of the well-to-do classes of the British population are born under the conditions which I have been supposing. The number of those who are the descendants of parents who have suffered from humoral gout is very great indeed, and, especially in our large towns, it is by no means restricted to the wealthy classes. We must be prepared, then, to encounter, not unfrequently, peculiar types of morbid action which derive their peculiarities from this association. It is the duty of the clinical investigator to carefully examine the evidence which is forthcoming as regards each before he admits its claim. Certain diseases of the eye for which this claim is with more or less confidence put forward will form part of our topics for discussion this evening. It may, perhaps, facilitate matters if I express at this stage the belief that most of the varieties of what is called "rheumatic gout" stand in this relation to true gout. The list would include lumbago, sciatica, neuralgia, crippling rheumatism, arthritis deformans, osteo-arthritis, many forms of iritis, and many cases of glaucoma. I by no means wish to suggest my belief that any one of these is, in all cases, in some degree dependent upon the inheritance of gout. Some of them are doubtless in not a few instances purely rheumatic, and this admission renders it desirable that I should say two or three words as to what our conception of rheumatism ought for the most part to be. Let me hint that from our primary idea of rheumatism I should like to exclude all causes having reference to food or assimilation, and to count only those which regard climate and weather, and especially exposure to cold and damp. The rheumatic diathesis, like that of gout, may become ingrained, and at the same time modified, by hereditary transmission, and in this connection it is easy to overlook its original causes and even to believe that it may begin spontaneously, or in consequence of other influences than the true ones. The hereditary diathesis of rheumatism

(arthritic susceptibility to weather) may easily co-exist with that of gout (arthritic susceptibility to diet), and each of them or both together may combine in endless variety with other forms of morbid tendency. It follows that we have to study many hybrid, or mongrel, types of disease in more or less close connection with them.

“HOT EYE,” IRRITABLE EYES, QUIET GOUT, &c.

There is a condition which, for want of a better name, I have for long been in the habit of recognising as Hot Eye. It is one of the many curious phenomena which attend quiet gout. I use the term quiet gout as distinct from acutely paroxysmal gout. In families liable to gout, for one person who becomes the subject of acute attacks of podagra—what is known as “unequivocal gout”—there are often half-a-dozen who are the subjects of minor symptoms which denote a similar tendency. In them the tendency never rises to sufficient height to induce a severe and characteristic paroxysm. They are nevertheless liable, after taking beer or wine injudiciously, as to quantity or quality, to experience slight pricking pains in joints, attended by lithic acid in the urine, and other unmistakable signs of the diathesis. In many, these symptoms occur in such invariable association with the causes suggested that it is not possible to doubt as to their nature. The liability varies with the weather and time of year, and it often ends, unless precautions are taken, in a sharp attack in the great toe. The terms “suppressed gout,” or better, “undeclared gout,” are often used in reference to it, but since it is simply a minor form of the disease, not in any way suppressed, and certainly not to trained observers undeclared, I prefer the term quiet. Amongst the frequent occurrences which denote its presence are repeated short attacks of congestion of the eye. Usually one only is affected, but sometimes both. The conjunctiva becomes red, and the eyeball feels hot, and pricks, as if sand were

in it. The attack may come on within half an hour of the meal which has disagreed, and it may last a few hours, or a day or two. Sometimes, owing to interference with the ciliary muscle, vision is slightly dim, and all attempts at accommodation are usually painful. Those who are liable to "hot eye" not unfrequently in the end suffer from iritis; indeed it is not unfrequently an introductory stage to that disease. On the other hand, many are liable for years to very frequent attacks of it without apparently any risk of its assuming more serious proportions. In the intervals of the attacks, the eye is usually quite well, and it is indeed its definitely paroxysmal character, its sudden development, and very complete and rapid disappearance, which especially mark its arthritic relationship.

In connection with this subject it may be noted, that not unfrequently those who suffer from unequivocal gout, experience shoots of sharp pain in the eyeballs, the nature of which they well understand. A man, William B—, æt. 51, who had suffered much from gout, and whose father and four of his father's brothers had all had it, complained much of this kind of pain, yet he had never had iritis. When he was gouty, he said he frequently had "darts of pain through the middle of each eyeball," and occasionally across the bridge of the nose.

A further practical note I must ask leave to make. It is to the effect that many cases of irritable, hyperæsthetic and easily tired eyes in young persons are in association with inherited gout. I feel sure that in some of these cases we are in the present day in danger of pushing the recommendation of spectacles to an excess. Some slight degree of hypermetropia may be detected, and it is assumed to explain the irritability of the eye. Yet glasses do no good, and in fact only increase the irritation. In such cases, very often, the real malady is inherited gout.

TRANSVERSE CALCAREOUS BANDS OF THE CORNEA.

I must admit, in the commencement of our investigation, that little or no evidence exists as to the deposit of urate of soda in any of the structures of the eye. I doubt whether it has ever been proved in any single case, although several observers have suggested its presence. I trust, however that we are long past the stage of belief which regards such deposit as essential to a gouty inflammation. The nearest approach to proof of gout deposit has been made probably in the case of what are known as the transverse calcareous bands in the cornea. Sir William Bowman, in the year 1848, was, I believe, the first to publish cases of this kind, but the earliest case had been observed by Mr. Dixon. Valuable information respecting the disease has recently been collected and studied by Mr. Nettleship. I believe I may fairly sum up the present state of our knowledge by saying that no one has yet proved that the salt deposited is that of gout. It is usually calcareous. The disease occurs in persons very likely to suffer from gout, and sometimes with a gouty history, but we are not as yet justified in holding that this earthy deposit is in direct connection with that diathesis.

ARTHRITIC IRITIS.

It will be admitted, without dispute from anyone, that there are several forms of *Iritis* which are arthritic in their origin. In other words, they occur to those who are liable to attacks of inflammation of joints, and the inflammations of the eye occur under conditions similar to those which excite the inflammations of the joint. Both are in some cases prone to occur in an acute but transitory attack. Both are very apt to recur after periods of complete immunity, whilst in other cases both may assume a

chronic, persisting, and destructive form. With such features of similarity we are justified in assuming, as indeed is done by all authors, that they are due to the same causes. The problem which I have to consider to-night is whether these causes belong to gout or to the rheumatic class ; whether they are sometimes the one and sometimes the other ; and lastly, whether they are not in some cases of a hybrid or mixed nature. Permit me briefly to state some of the peculiarities which are to be observed in different types of arthritic iritis ; and to ask respecting each what evidence we possess which would connect them with the tendency to gout. Let us take first the common form in which the iritis occurs in acute transitory paroxysms. Of this, good typical examples will be in the memory of us all. Its subjects are usually men, and often of vigorous health, and past middle life. The iritis scarcely ever affects both eyes at the same time, but occurs first to one and then to the other, sometimes keeping to the same eye during several successive attacks and then leaving it, to assail its fellow. The attacks are often acute, beginning very suddenly, and attended with great pain. When they subside they subside completely, and leave the eye without the slightest degree of irritability. The duration of the severity of an attack may vary greatly at different times in the same patient. The tendency to form adhesions is extremely great. It is very remarkable how, in most cases of this class, the disease restricts itself to the iris and shows no tendency to disorganise the globe. Thus a patient in whom, in consequence of repeated attacks, the pupils may be occluded, with the exception of mere pin-hole apertures, may yet continue to enjoy very fair sight. I by no means wish to imply that this is usually the case. The case of Dr. Curry, as recorded by himself in the ' *Medico-Chirurgical Transactions*,' is a good type-form of this disease. Dr. Curry had suffered much from true gout. I may cite, however, as another good type-illustration of this class, that of a gentleman whose friendship I had the privilege to enjoy during a long period of

my early life. He was a florid, fair-complexioned man, of vigorous habits, a large eater, and through life accustomed to drink beer. He inherited rheumatism, but there was no known history of gout. He had suffered in boyhood in consequence, as he believed, of too free bathing when hot, from rheumatic fever. When I knew him first he was about five and forty, and from that time onwards to his death, at the age of sixty, he had a variety of arthritic complaints—lumbago, sciatica, chronic arthritis of the knees and other joints, and recurrent iritis. Both his pupils were closed with the exception of very small apertures, through which, however, he still continued to see enough to follow an occupation in which everything depended on good sight. I attended this gentleman year after year, usually in the early part of February, for most severe attacks of iritis, never affecting both eyes at once. He was benefited by large doses of iodide of potassium, and the free use of blisters. At length his attacks ceased to occur, and during the ten years preceding his death I believe he had not had any. I made a post-mortem examination of his body, and found no proof of gout. His knees contained loose cartilages, and there were patches where the articular cartilage was wearing away, but there was no urate of soda.

I have the record of a considerable number of cases in which the liability to these recurrent forms of iritis occur in those who were the subjects of unequivocal gout. But there were a far larger number in whom the concomitant symptoms were of rheumatic arthritis and not of true gout. The question remains, however, whether this form of rheumatic gout, although unattended by lithate of soda deposit, be not in reality a hybrid disease possessing, in addition to an unquestionable share of rheumatism, an admixture also of gout tendency. The infrequency of the disease in women, the age, the habit of body, and the mode of life of those who are usually its subjects, seem to me to strongly support this view. So does also the extreme severity of the attacks, and their rapid and

complete subsidence when once the point is turned. When this form of iritis does occur to women it is never, I think, so acute or so paroxysmal as in men; and it almost always happens to those whose relatives have suffered from gout.

VARIOUS FORMS OF ARTHRITIC IRITIS.

Next to this form of recurrent iritis, and including perhaps some of its examples, I have to mention the iritis which occurs in connection with gonorrhœal rheumatism. All observers will admit, I think, that iritis associated with ordinary rheumatic fever is infinitely rare. So also is it in connection with that type of arthritis to which the name of crippling rheumatism may be given. In other words, iritis in association with either acute or chronic rheumatism is a thing that we scarcely ever observe. In men, however, the rheumatic affections, whether acute or chronic, which are induced by gonorrhœa are not unfrequently accompanied by iritis. This iritis is less distinctly paroxysmal and much more liable to persist and be destructive than is the case in the form which I have just described. My explanation of this is easy. The reason why gonorrhœal rheumatism so often causes iritis is because it occurs usually to the subjects of inherited gout.

A third form of arthritic iritis is one which affects women more frequently than men, which not unfrequently attacks both eyes at once, which is apt to spread to the ciliary region and choroid; to persist and to prove destructive. In a very considerable proportion of the women who suffer from this form there is a history of gout in former generations.

Another group of arthritic iritis might be constituted of cases in which the malady happens to young patients, and proves but slightly, if at all, liable to recur. These single attacks of iritis without history of gonorrhœa or syphilis, and occurring in young persons, usually, I think,

affect the male sex and almost always there is the history of gout in relatives.

I must just mention, in order to complete my classification, although it is but of little clinical importance, the group of cases in which iritis occurs in association with glycosuria. In these, according to my experience, the patient is almost always the subject of gout also.

To sum up then, I would say, that although in many individual cases of arthritic iritis there may be no proof of liability to gout in either the patient or his relatives, yet the tenor of the evidence in general is in favour of the conclusion that when iritis occurs there is in reality some gout complication. The more purely and definitely is the case one of rheumatism the less the probability that iritis will happen. When iritis occurs, the complications are almost invariably those which suggest what we call rheumatic gout, rather than rheumatism pure and simple; thus the smaller joints are often affected and *nodi digitorum* present, whilst *sciatica*, *lumbago*, and *neuralgia* are frequent complications. I have elsewhere tried to prove that gonorrhœal rheumatism occurs, in nine cases out of ten, to those who inherit a gouty constitution and that it is chiefly this inheritance that gives the proclivity to it. I cannot stop now to recapitulate the evidence on which this belief rests, but if it be trustworthy it offers an explanation of the fact that iritis is so frequently met with in association with this type of rheumatism.

In certain cases of iritis with arthritic associations a very peculiar condition is observed. I allude to the filling of the anterior chamber with a soft, gelatinous, jelly-like mass. This material, which produces an appearance most alarming to the uninitiated, concealing the pupil and suggesting entire opacity of the cornea, is susceptible of very rapid absorption, and may, in the course of a very few hours, clear right away. I believe that in some cases of syphilitic iritis this peculiar form of effusion has been noticed, but if I were to speak from my own experience I should cite it as a symptom very strongly indicative

of gout. I would make the same remark, though with less confidence, respecting the rare cases of iritis complicated with hæmorrhage.

STATISTICS AS REGARDS ARTHRITIC IRITIS.

My report on arthritic iritis, published in the 'Ophthalmic Journal' of 1872-74, contains the narrative of 104 cases which are suitable for our present purpose; for various reasons I omit a few on account of doubts as to their nature or imperfections as to the details. The report comprises cases of all varieties in which either rheumatism or gout, or the two together, were supposed to have been the causes of inflammation of the eye. They were collected from my note-books, both from hospital and private practice, up to the date mentioned. It is right to state that at that time my opinions as to the influence of true gout were less definite than they are at present, and that my knowledge as to the means of recognising it was also less. The notes of many of the cases had been taken some years before they were published, and thus certain inquiries which I should now carefully make were unfortunately omitted. On this account I have no doubt that we must consider that the report does not show the influence of gout so strongly as it would have done had the cases been taken with better knowledge. Out of the 104 cases I find a positive history of gout having occurred in the patient in only eighteen cases, and only in twenty-nine was there the history of positive gout in near relatives. It is to be observed, however, that these numbers take cognizance only of what has been termed "unequivocal gout," namely, of those in which an unmistakable attack of inflammation in the great toe or other single joint had unquestionably occurred. It omits altogether the much larger number in which the gouty tendency had remained undeclared, or had been manifested only in what we may

term its quiet form. Anyone who will examine for himself the narratives to which I refer, will, I feel sure, agree with me in this opinion. In a very large proportion there was, I think, good reason for believing that a taint of gout existed where the narrative mentions rheumatism only; the arthritis had constantly been of the chronic type, and had very frequently presented exceptional conditions. If we may count such maladies as sciatica, lumbago, neuralgia, nodi digitorum, gonorrhœal rheumatism, rheumatic gout when affecting the smaller joints, as being for the most part indications of gout rather than of pure rheumatism, we shall then find that facts at any rate suspicious of gout are present in nearly all the cases. It is, I think, decidedly exceptional for rheumatism pure and uncomplicated to show any tendency to attack the tissues of the eye. Iritis in conjunction with rheumatic fever of the ordinary form is almost unknown, and even in cases in which patients suffer from repeated attacks, as is sometimes seen, the eyes usually remain free. So soon, however, as changes of type occur, the disease showing a tendency to attack the smaller joints, or to affect only single joints, or to pass into the chronic form; then we get the liability on the part of the eye to suffer. On this point my own experience is quite in accordance with that previously expressed by an acute and zealous clinical observer, the late Dr. Fuller. Dr. Fuller in his first edition, speaking of rheumatic gout, stated that in 11 out of 101 cases which he had tabulated when Medical Registrar to St. George's Hospital, the eye had suffered more or less severely, and in 14 out of 193 cases which had subsequently been under his own care in the hospital. In a second edition he stated that he had so far modified or developed that opinion that he had come to believe that many, if not all, of the cases in which this complication occurred, were examples of obscure gout or else of gonorrhœal rheumatism, adding, "This at least is certain, that since my attention has been specially directed to this

question, I have been enabled to find a gouty or venereal (meaning, I think, gonorrhœal) taint in every case in which the eye has been inflamed in connection with presumed rheumatic gout." This is strong testimony, stronger certainly than I am prepared to give, but I doubt whether it much exceeds the truth. Among other authors who have distinctly recognised gout as a cause of diseases of the eye, and appear to have referred a considerable proportion of those diseases, when arthritic, to gout rather than to rheumatism, I may mention Dr. Jacob, Mr. Weller, Mr. Middlemore, Dr. Copland, and perhaps Mr. Wardrop. Mr. Middlemore goes so far as to think that he has seen in one or two cases the pupil occupied by calcareous concretion of a gouty nature. None of these observers have, I think, recognised any other special disease as connected with gout than recurrent iritis. Almost invariably I find sclerotitis put down as rheumatic.

INSIDIOUS AND DESTRUCTIVE IRITIS FROM INHERITED GOUT.

I must now enter upon one of the most important divisions of my lecture. It is the attempt to prove that there is a very peculiar form of destructive iritis, occurring for the most part in young persons, which stands in all cases in direct relation with the inheritance of a gouty constitution. It does not occur to those who themselves suffer from attacks of gout, but to their descendants.

It was in 1863 that I first saw, at Moorfields, a girl named Mabey. She was then about eighteen, tall, well-formed and florid. She was too florid, and the circumscribed areas of colour in her cheeks varied in tint from bright red to slightly livid, according to the coldness of the day. She was the subject at the same time of a most peculiar form of arthritis of the last joints of all her

fingers, and of double iritis. Her right eye was lost and painful and I accordingly excised it. Her left was saved only by repeated iridectomies and finally by extraction of the lens. After the last operation she continued for five years to enjoy good sight and was free from relapses. Her fingers also got well. At the age of twenty-six she became the subject of phthisis and died, I believe, at about thirty. I showed this patient at the Hunterian Society, and drew attention to the unusual features of her disease. Someone present, looking at her hands, remarked, "Surely this is gout." I took the hint, inquired into her family history and found that in all probability it was gout. Her father had suffered repeatedly from that malady in an unequivocal form, and he had tophi in his ears. I show you drawings of the state of the girl's fingers. In order to ascertain whether there were urate deposits in connection with the swellings, I cut into one of the largest and obtained only a soft jelly-like substance. When, after some years, the swelling subsided, the terminal joints were all left disorganised and the last phalanges were more or less displaced. There appeared reason to believe that a feeble state of the circulation combined in this case with the gout inheritance.

I have repeatedly mentioned Mabey's case in public and with it others which came under my notice subsequently and I must content myself on the present occasion with a short summary of the facts. In 1872 I gave a lecture at Moorfields on this disease and was able to cite four well-marked examples of it. In 1880 I gave, at the London Hospital, a second lecture, in which I adduced seven additional cases, making eleven in all. Of these eleven patients four were females and seven males, but it happened that the most typical and severe forms of disease occurred in females, a fact which my subsequent experience has fully confirmed. In all cases both eyes ultimately suffered, but only in one were both affected simultaneously. In three instances "last-joint arthritis,"

that is to say destructive inflammation of the last joints of the digits, occurred. In all the cases vitreous opacities as well as iritic adhesions were developed. In most of the cases the eye which was last attacked suffered most, and a decided tendency was shown for the disease to come to an end as the patient advanced in life. In several cases the eye was saved by repeated iridectomies, and in one or two change of climate seemed to be very beneficial.

In all the cases but one the family history of gout was clear and strong, and in the exception the patient was the son of a brewer's man, and his father had died early, so that it was very possible that his proclivities had not declared themselves. In several of the cases the patient had been reduced almost to blindness. In three one eye had been excised and in one both were quite lost. In all excepting two the disease had begun between puberty and the age of twenty-five, and it would appear to be the fact that the earlier it begins the more severe it is. Such then was the state of my facts in 1882 when I put my last lecture into print. I think you will allow that I was justified in avowing a strong belief that inherited gout was the real cause of this peculiar form of iritis.

That I may add a little more colour to my picture I will ask your permission to state briefly the facts of three cases which have come under my notice since the date referred to. They are all cases of great interest and two of them illustrate a fact which I had previously only once or twice noticed, namely, that there is a tendency to the formation of cataract.

The last case which I have seen is that of a Miss D—, æt. 30, the daughter of a surgeon in the country. She was brought to me with the left eye quite destroyed, and the right nearly so. In the left an opaque lens had undergone spontaneous absorption and the iris was everywhere adherent to the opaque capsule. In the other the lens was half opaque and the iris extensively adherent. I was told that I had myself seen this patient ten years

before and had then recognised only commencing cataract in both. The patient's father, an intelligent medical man, was astonished when I told him there had been extensive iritis, and assured me that his daughter had never had any attacks of inflammation. The patient herself said that she had never noticed more than that the eyes had sometimes been red and hot and would prick a little. We have here then a good instance of the very insidious course of the disease. In the left eye there was no perception of light, and very probably the vitreous was affected. I found on inquiry that in Miss D—'s family there had been much gout. She herself appeared to be in good health.

Mrs. O— is a young married lady who has borne three children, and during lactation in each instance suffered from a chronic form of almost painless iritis. Both pupils are almost excluded. Her father and several other near relatives have suffered from gout. She is of feeble circulation and liable to chilblains.

I have kept one of my best cases to the last. Miss L—, æt. 22, is the youngest of a family of eight, and was born after the death of a father who had suffered much from "chalk gout." Her eldest brother has had both gout and rheumatism, and there is rheumatism also on her mother's side. Miss L— began to suffer in her eyes at the age of twelve, and at first it was simply redness and irritability with "black specks," "balls," and mist. At the age of fourteen she had a severe attack in both and was several months under Mr. L—'s care "almost blind." From this she recovered, but at eighteen a surgeon in Liverpool performed an iridectomy on the left on account of exclusion of the pupil. A little later Mr. C— did an iridectomy in the right. Neither of these operations resulted in any improvement of sight. In 1880 a surgeon in Leeds removed a soft cataract. The eye was lost and Mr. C— excised it a few months later. Miss L—'s present condition is that with the one remaining eye she can just puzzle out $\frac{20}{200}$.

Could I possibly produce before you a more marked example of an insidious destructive disease, progressing in spite of the most highly skilled assistance to its melancholy end? Yet it is only a fair example of about half those included in my series.

RELAPSING CYCLITIS.

There is a peculiar form of chronic inflammation of certain parts of the eyeball which clinical observers have recognised under the name of *Relapsing Cyclitis*. It is a cyclo-kerato-iritis involving the ciliary region of the sclerotic, the adjacent part of the cornea, and the iris. Sometimes one of these structures suffers more than another, but usually all are involved. It seldom damages the pupil itself much, seldom invades the centre of the cornea, or only very late in the disease, and it does not show much tendency to involve the choroid or vitreous. It usually begins in one eye and only affects the other after a long interval, and to the last one usually suffers more severely than the other. It may begin in early life, but often does not do so till middle periods. When once it has begun it never wholly leaves its victim, but continues either to persist with slow chronicity or recurs over and over again after intervals of health. It causes scars in the ciliary part of the cornea, thinning and discolouration of the ciliary region of the sclerotic, and ends either by inducing staphyloma or by making the whole cornea dull. It may occasionally become needful to excise the eyeball on account of the persisting irritability combined with great impairment. It affects, I think, women more frequently than men. It is a very peculiar type of disease, and it would not be difficult to place side by side a group of examples of it all exactly alike. I know of no treatment short of a complete change of climate which does much to benefit it. In this respect it is much like the form of iritis which I have been striving to prove to be a

direct result of inherited gout. Yet I am by no means in a position to produce before you, respecting relapsing cyclitis, such evidence in reference to its gout origin as that which we have been discussing. I dare not, indeed, do more than suggest that it is very probable that in some cases the constitutional cause of this destructive and persisting disease may be gout. I have found a gout history in some of my cases, but in others it has been absent. The last case which I have seen has been the one in which this suspicion was the strongest.

Miss D— is the daughter of a medical friend. She is now forty, and she has suffered all her life from her eyes. She was treated when two years old by Mr. McMurdo for what was called strumous ophthalmia, and which proved very intractable. Since then she has had numberless relapses, and in both eyes the cornea at its edges has become extensively opaque. Her left eye is the worst, and has recently been so troublesome that I was inclined to recommend its excision. It is by no means a blind eye, but the cornea is so hazy that it is of little use. Now, say that we have here a case in which strumous ophthalmia has persisted through life, let us ask what is the constitutional peculiarity which has conduced to this exceptional result. The lady has been well cared for all her life, she shows no other signs of struma, nor are such present in any of her relatives. Her father is a robust man. Mark that both eyes have suffered, and that in both it is the peripheral parts of the cornea which have been chiefly involved. Now, this lady's maternal grandfather and two of her maternal uncles suffered severely from gout, her mother had an attack in her great toe, and even she herself has on one occasion had an attack of it. This strong gout history is certainly by far the most definite fact which I can obtain as explaining the peculiarities of her eye disease.

CAN GOUT CAUSE NEURITIS ?

Although from time to time strong suspicions have been expressed as to the occurrence of gouty affections of the nervous system, I believe the question has seldom been definitely put, "Does gout cause neuritis?" We know that gout can cause the tissues of a joint to inflame, and that cedematous effusion with cell proliferation, in fact the ordinary phenomena of inflammation, are its results. I shall be much disappointed also if it is not regarded as proved that it can cause the tissue of the iris to inflame in a precisely similar manner and with like results. Can it attack any of the structures of which the nervous system is composed? Can it cause inflammation of ganglia, of nerve-trunks, or of their investments, or may it indeed attack the central organs? Without venturing to suggest that any of these occurrences are common, it is yet difficult to deny the possibility or even probability in exceptional cases. We can understand the influences that are likely to localise gout; its exciting causes may come into play far less frequently in reference to the nervous system than to the joints; or even to such an apparently exposed part of the organism as the eye. Still, if we admit that, in the gouty condition, inflammation may attack the cellular tissue in any part of the body, it is difficult to suppose that the cell elements which enter into the formation of nerve-trunks, for instance, will always escape. I am glad to note in the progress of neuro-pathology of the last few years that there seems an increasing tendency to recognise the possibility that not a few nervous affections may be due not to central disease, but to primary inflammation of the connecting trunks or of the peripheral organs. We know for certain that chronic neuritis, probably beginning peripherally, is a part of leprosy. It is highly probable that it is often a part of syphilis and quite possible that it occurs also in locomotor ataxy. How else explain many of the transitory forms of ptosis, &c., which occur in the two latter diseases?

GOUTY NEURITIS OF THE OPTIC NERVE.

The question which I now wish to put definitely is this : Are there any cases of inflammation of the optic nerve, or of any of the motor nerve-trunks of the eyeballs, the direct cause of which is the existence of a gouty constitution ? The decision is of importance not only in reference to diseases of the eye, but because it would throw much light on certain other obscure affections met with in this disease. Is sciatica really a neuritis of the sciatic trunk or its sheath ? Are any of the forms of gouty neuralgia really produced by gouty neuritis ? If the optic nerve can inflame from gout why not the pneumogastric or the phrenic ? I am sorry to say that I have exceedingly little evidence to bring forward in answer to these important questions. I will mention briefly one or two facts which are suggestive, and which I hope will prove sufficiently so to induce a more careful investigation of the subject. In two or three cases I have attended young ladies, of families in which gout had been prevalent, who suffered from attacks of ptosis, with the other group of symptoms referable to paralysis of the third nerve, for which I could find no other more plausible explanation than that they were caused by gouty neuritis. In one of these the attacks were transitory and recurrent, several having occurred within a period of a few years. I must not venture to trouble you with details of these cases, but feel myself obliged to speak rather more at length concerning one in which the optic nerve itself was inflamed, there being, I think, fair reason to suspect that the cause was inherited gout. A young lady of sixteen, was brought to me from Ipswich, on account of blindness of one eye, in July, 1879. She had no other symptoms whatever, was florid and appeared to be in good health. The ophthalmoscope showed nothing but what was quite normal, and had it not been that her pupil dilated more widely when the other was covered, I might have suspected

that she was feigning. A week later the changes were conspicuous, the disc being much swollen and its margin concealed. Under treatment by mercury combined with quinine, in the course of a few weeks the attack had passed away, but the disc was left pale. Four years later I saw this patient again with similar symptoms in the other eye, and I was now told that she had several times suffered from transitory attacks which always occurred during spring. Her health remained perfect. There was a strong history of gout in the family, both her parents having suffered repeatedly. It seems to me not at all improbable that these recurrent attacks of transitory neuritis have been really of a gouty type. In saying this, I have regard to the entire absence of the more ordinary causes and concomitants of optic neuritis, to the recurrent nature of the affection, its want of symmetry, and the exceptionally strong family history, both parents and many other near relatives having suffered. I cannot call to mind any other case of recurring optic neuritis, with gout history, which I could at all fairly place side by side with this one. I have, however, several times seen optic neuritis occur in women who were of gouty family without any of its ordinary accompaniments and clear away entirely under treatment. In none of the cases of which I am thinking did any recurrence take place, and in most of them both eyes were simultaneously attacked.

OTHER FORMS OF GOUTY NEURITIS.

Before quite taking leave of this important question as to the possible occurrence of gouty neuritis, I must trouble you with yet one other case. A lady of fifty-six came under my observation in whom for several weeks the left facial nerve had been quite paralysed. I observed at once that she had had two large iridectomies done downwards and outwards, and on inquiry found that she was nearly blind. It was twenty-three years

since she had first noticed cobwebs before her sight, and fifteen since she had last been able to read. The iridec-tomies had been done six years, with what object I do not know, for although she said that she had once had rheumatic inflammation of her eyes, there did not appear to be any adhesions. In each eye the lens was partially opaque, and in each there were extensive choroido-retinal changes with pigment deposits and waxy atrophy of the discs (a peculiar form of retinitis pigmentosa). Mrs. H— told me that she had a sister who also had “amaurosis.” I persuaded her to bring her sister, and may now state the family history which belongs to them both. One of their brothers suffers not unfrequently from attacks of unequivocal gout, and a sister is crippled by rheumatism, but their parents died aged, and are not known to have suffered. The elder of the sisters, aged sixty-nine, has herself had gout, and describes her great toes graphically as having been swollen and “red like tomatoes.” This sister after her first confinement, aged twenty-seven, had an attack in her eyes which lasted several years attended by great pain and intolerance of light. Her recovery from it was very gradual, but finally it was almost perfect, and I could now find no material changes. For several years she has been very deaf and she has suffered severely from neuralgia and chronic arthritis of wrists and fingers. Once she was told by Mr. Critchett that she had “gout in the eyes.” The younger sister describes ten years ago an attack of shoulder neuralgia attended by torticollis which kept her in bed six weeks and was accompanied by agonising pain, as if her arm were in the fire. She also has suffered most severely from neuralgia on many occasions.

Here then is a family so definitely gouty that a brother and sister have each had true gout and several other sisters are crippled by rheumatism ; one sister has become almost blind with neuro-retinitis, and has suffered from neuralgia, torticollis, facial paralysis and an attack which was probably neuritis of the brachial plexus. Another sister is

deaf, has been all her life liable to neuralgia, had a several years' attack of pain in her eyes which was called "amaurosis," and in later life one which was named "gout in the eye." It seems highly probable that we have here an instance of gout affecting, at different periods of life in each sister, different parts of the nervous system, and attended by true neuritis of various nerve structures.

GLAUCOMA IN RELATION WITH GOUT.

I had intended to have said a good deal as to the nature of glaucoma and the evidence as to its connection with gout. The subject is, however, too important to be discussed hurriedly, and I have not time to attempt to do it justice. I must therefore content myself with the mere expression of opinion that in the production of this extraordinary disease a gouty tendency often takes a large share. More particularly would I suggest this in the exceptional cases in which glaucoma shows itself in early life, and I could, if time permitted, bring before you some important cases illustrating this point.

RETINITIS HÆMORRHAGICA AND ITS CONNECTION WITH GOUT.

There is yet another definite and well-characterised affection of the eye which claims conspicuous mention in connection with our subject.

Retinitis hæmorrhagica is very rarely indeed seen excepting in those who are themselves gouty. It is a disease of middle life or of advancing years, and in its more typical forms is never seen in the young. It seldom happens to both eyes at once. It is attended by great swelling of the disc and adjacent parts of retina and by such turgidity of the central vein that I was at one time tempted to believe that it depended primarily upon throm-

botic phlebitis of that vessel. It is possible, however, that such is not its true pathology, and that the venous distension and stasis, perhaps in some instances thrombosis, are really secondary to the neuritis. If this be the fact and neuro-retinitis be the primary condition, then we have in retinitis hæmorrhagica an instance in proof that acquired or humoral gout may become the cause of neuritis. It is not, I think, ever seen in association with the inheritance only of a gouty constitution, but is found usually with lithæmia, in free livers who have experienced unequivocal attacks. The case which Jaeger took for his beautiful plate in illustration of the disease, was that of a tavern keeper of plethoric habit and bloated appearance. Nothing is said as to gout, but it is fair, in such a man, to assume that there was a tendency to it. In order to determine statistically, as far as practicable, the association of retinitis hæmorrhagica with gout, I have tabulated twenty-four cases—with the exception of the one of Jaeger's, all from my own note-books. In twelve of these, exactly one half, the patient had suffered definite attacks of gout, and in five others there was strong presumptive evidence of a gouty constitution. In seven there was no proof of gout. This last group comprises two in which diabetes was present; one in which albuminuria existed, and two in which the retinitis was not very well characterised as of the hæmorrhagic group. If I had kept more closely to the type of cases illustrated by Jaeger's portrait, I should have been able to make yet stronger statements as to the almost invariable association of the disease with gout. It may be of interest to state a few other facts deduced from my table. Thirteen of the patients were men and eleven women. The youngest was forty-five. In seventeen cases only one eye was affected, and in seven both. In some cases there were hæmorrhages only, with little, if any, evidence of neuro-retinitis.

SUMMARY.

I must now bring my lecture to a close, and in doing so may, I think, venture to assume that it has been shown to be probable that there are many different forms of inflammation of the eye, or of parts of it, which are in connection with gout. Some of these are very peculiar and specialised types of disease, and have already been accorded distinctive clinical names; others quite as distinct are not as yet so well known, and of others we may say that they are to be distinguished from other inflammations of the same structures not so much by their features as by their cause. Of all we may assert that they are infrequent; some, if we confine ourselves to well-marked types, are distinctly rare. We have divided these different affections into two groups: (1) those who go with acquired, humoral, or renal gout, and (2) those which depend upon the inheritance of structures damaged, or at any rate specialised, by gout in predecessors. It is needless to repeat that in almost all cases of acquired gout there is inheritance also, and that in many in which the disease is chiefly caused by inheritance, some modification and increase may have been derived from personal habits. Still, the difference between the two classes of affections is very marked. In the one attacks of a transitory nature are the rule, and these attacks are often acute and attended by much pain. In the second group, although a tendency to temporary recovery and recurrence is often observed, yet there is a great proneness to chronicity and persistence. The invasion is often insidious, but the disease is usually in the end destructive. In the former group we have placed hot eye, sclerotitis, recurrent iritis, and retinitis hæmorrhagica. All these are diseases of adult life. In the second group we have insidious disorganising iritis, relapsing cyclitis, certain forms of soft cataract, and perhaps some of primary optic neuritis. Not only are there clearly marked clinical differences between the two

classes of affections, but the difference in treatment is equally marked. In the first, the well-known measures against gout must be taken, a restricted regimen, alkalies, colchicum, aconite, and liberal counter-irritation. In the second we must use tonics, and although counter-irritants are here also often valuable, we cannot trust to any measure as really curative short of complete change of climate.

ON THE PROOFS OF GOUT.

It may be, perhaps, convenient to say a few words as to the kind of evidence which justifies a diagnosis of gout as the cause of any particular disease of the eye. In the case of humoral or acquired gout there ought to be the history of one or more definite attacks of joint inflammation usually of an acute character, and attended by redness and œdema, and followed by peeling; usually the great toe will have been the joint affected. Such patients will often state that they are very susceptible to the influence of beer and wine, and that malt liquor and some wines almost always cause indigestion, and make the urine muddy. These dietetic disturbances, to which as a test of gout attention was, I think, first claimed by Sir James Paget, are very important and valuable. If tophi are present in the ears or elsewhere they are of course conclusive. In a few cases we are justified in assuming the existence of humoral gout, although no paroxysm has ever occurred. If the dyspepsia be there, if the joints ache and prick after beer or wine, and if there be gout in relatives, we may confidently believe that it is present, although not yet declared. As regards the inherited form, we may take it as highly probable whenever parents or grandparents, or any one of them are known to have suffered definitely. If even uncles, aunts, brothers, or sisters, or cousins have suffered from true gout in early life, the belief that a family taint exists becomes very probable. The evidence must always be carefully sifted.

It will not do to take the statement of the patient without first carefully informing him as to the scope of the inquiry. Patients will often confess to gout who do not know what the word means, and a far more numerous class will hastily deny its history, although the facts, when correctly obtained, may be most conclusive. If, however, proper care be taken, and the patient, after being instructed, be allowed time for consideration—above all, if the inquiry be repeated after an interval, or if several relatives be interrogated, then I believe that in most cases truthful data will be obtainable.

It may be inquired as to the value of certain affections which may be considered to belong both to rheumatism and gout, as symptoms of the latter. Permit me very briefly to repeat my creed. I believe that the subjects of gonorrhœal rheumatism are in a very large majority of instances the inheritors of a gouty constitution, and that all the conditions usually classed as rheumatic gout are really, in most instances, dependent in a large degree upon like inheritance. Thus, if a patient has had sciatica or lumbago, if he shows *nodi digitorum* (osseous, not tophi), if he has suffered from chronic rheumatism affecting the smaller joints, I should think it fair to allow considerable weight to these facts as pointing to a taint of gout. Even the strictly rheumatic disorders, acute rheumatism itself, if it happens to the relatives of those who have had gout, lends support to the theory of family tendency to gout. It is an observation as old as the days of Heberden, and confirmed I am sure by daily experience, that the children of the gouty are more liable than others to attacks of rheumatic fever. In this we see another proof of the inheritance of structural proclivities, rather, I think, than of blood disorder or tendency to it.

CONCLUSION.

And now, gentlemen, as my last word, I do not know whether I have to defend myself in the eyes of any of

you from the charge of "seeing gout in everything." I am well aware that this diagnosis is a very easy one, and seductively ready at hand for the idle prescriber. I submit, however, that it has not been exactly in that temper that I have brought before you the statements which I have made this evening. My desire has been to state the issues explicitly, and to keep close to facts. Where statistics were admissible and obtainable I have had recourse to them. I may assert that I have said nothing but what has been based not only upon clinical observation, but upon clinical note taking, and the subsequent collation of cases. Nor, I contend, have I made any very sweeping statements. So far from my having exceeded the truth, my conviction is that when our clinical pathology shall be more advanced, and diseases more minutely classified, we shall in all probability recognise as gouty yet other maladies, and perhaps not a few beyond what I have claimed. It is a subject upon which scepticism is as irrational as credulity. That the gouty constitution exists, and is very common in our English population, that it is potent in the production of disease, and that it is remarkably hereditary, are facts which no one will doubt. In relation to the multiform diseases of the eye it must have a domain, and that an important one. To discover some of the extensions and limits of that domain has been the object of my best endeavours to-night.

ON
HIGH AMPUTATION FOR SENILE
GANGRENE.

BY

JONATHAN HUTCHINSON, F.R.S.,
EMERITUS PROFESSOR OF SURGERY TO THE LONDON HOSPITAL, AND
PRESIDENT OF THE OPHTHALMOLOGICAL SOCIETY.

Read December 11th, 1883.

[*From Vol. LXVII of the 'Medico-Chirurgical Transactions,' published
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GANGRENE.

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JONATHAN HUTCHINSON, F.R.S.,
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Received October 9th—Read December 11th, 1883.

MY wish in writing the present paper is to claim the attention of the Society to the advantages of amputation high up in all suitable cases of senile gangrene. The practice of amputation for this disease has I believe been almost universally discouraged from the belief that it is generally followed by sloughing of the stump. I wish to urge that this occurs only when the part is removed too near to the disease. If done low down then I fully admit that the condition of the vessels will rarely be found to be such as to admit of repair, and that gangrene of the stump will usually occur immediately, and place the patient's life in much more danger than before.

As regards these operations I have had but little experience, having long believed that they were most unadvisable. The few trials that I have made have fully confirmed this opinion, and I would abstain as scrupulously

in senile gangrene as in frost-bite, from any interference with the knife near to the dying part. If, however, we go much higher up and amputate through parts which are still well supplied with blood the results are quite different.

By operations of this kind I believe I have repeatedly saved the patient's life. By "amputation high up," I mean in the case of gangrene of the foot, above the knee, and in that of the hand at or near the shoulder-joint. By senile gangrene I mean chiefly gangrene in connection with calcification of arteries, but I wish to be understood to include all cognate conditions, that is, all in which the death of the part occurs in connection with slowly progressive interference with the supply of blood. In the strictly senile forms this interference is usually greatest in the distal part of the arterial system, and it is of a nature to be steadily on the increase. Hence the hopelessness of improvement and the great danger of advance. It is impossible that any collateral circulation can be established.

In cases of gangrene from occlusion of the main trunk without disease of the smaller branches, as for instance from embolism or from ligature, the conditions are different. Here there is hope that, when the collateral circulation shall have become established, repair will take place with exfoliation of the sphacelated parts. Yet there are some cases, even of this kind, for which I think high amputation may become advisable. Certainly, amputation near to the gangrenous part should always be avoided unless, indeed, when after repair has considerably advanced there is proof of well-restored circulation. Cases of frost-gangrene stand in some sense midway between these two classes; they are examples of gangrene from distal suspension of circulation as in the senile cases, but the cause is not, as in the latter, a permanent one. There is hope of restoration of the supply of blood. Nevertheless, all authorities are, I think, agreed that this restoration is never sufficiently perfect to make it wise to attempt the removal of the affected parts,

and that it is necessary to leave this almost absolutely to natural processes.

Before proceeding to narrate my cases it will be well that I should here introduce a brief summary of what has been done by others in the same direction.

The proposal to amputate high up in cases of senile gangrene is not absolutely novel. In fact a discussion which bore closely upon it took place in this room more than thirty years ago. On that occasion the late Mr. Garlike of Rickmansworth brought forward a case¹ in which he had amputated in the upper part of the thigh, in a man aged 69, who suffered from senile gangrene in his foot. There was ossification of the femoral artery, and there could be no doubt as to the real nature of the gangrene. The case, however, did not imply any proposal to amputate high up as the best treatment for gangrene of this kind, since it was only determined on after suppurative destruction of the knee-joint, when there was no longer any choice as to the site of the operation, and could be but little difference of opinion as to its absolute necessity.

The case, notwithstanding, is a good example of complete recovery under the somewhat desperate conditions referred to.

In the discussion which followed, Mr. John Adams, my late colleague at the London Hospital, mentioned a case of recovery after amputation for senile gangrene, but the precise part at which the amputation was done is not specified. Mr. Fergusson mentioned that he had operated in a single case, but without success, and spoke with decided disparagement of the practice. To Mr. James, of Exeter, belongs, I believe, the credit of having definitely proposed the line of practice which I shall endeavour to support. Indeed, I believe in one case that he actually did what I only proposed to do, but was not allowed by my patient to carry out, viz. to amputate both the lower extremities. I cannot find that any surgeon, writing since this discussion took place, has recom-

¹ 'Medical Times and Gazette,' 1853, vol. i.

mended the practice, or even thought it worthy of serious consideration.

Even before the date referred to, Mr. Langstaffe and Mr. Guthrie had each of them in single cases amputated above the knee for senile gangrene of the foot. Mr. Langstaffe's patient died in consequence of the hæmorrhage within twenty-four hours, but in Mr. Guthrie's case the stump was almost healed when death from exhaustion occurred. Mr. Mott had, I believe, also done a single operation of the same kind.

Thanks to modern improvements in the details of operations and the management of wounds, the surgeon of to-day approaches the subject with great advantages.

As regards the risk of hæmorrhage during the operation the use of Esmarch's bandage unquestionably gives much real assistance. The calcified arteries, even those of the smallest size, are very easily found, and if tied with their accompanying veins in one loop and with a moderately thick ligature, I believe no difficulty will be found in making them secure.

Some years ago I had a case in the hospital in which the conditions causing the gangrene were complicated and in which the safety and advantages of high amputation were very strikingly exemplified. The patient was a strong-looking man of not more than thirty who had served as a soldier in Canada. There he had suffered from frost-bite of the left foot. It was not, however, followed by immediate gangrene, and a year later he came into the London Hospital with an ulcer on the foot which had destroyed several toes, which would not heal, and which every now and then showed a tendency to spread at its edges by fresh sloughing.

After he had been some time under observation my colleague, Mr. Little, during the summer vacation amputated through the tarsus. When I resumed duty I found the face of the stump gangrenous and the condition of things worse than before. It was now discovered that the femoral artery in the thigh was obliterated. I decided to

amputate again and to do what I then considered a high operation. The leg from the ankle upwards was perfectly healthy, and I amputated just below the knee. The result, however, was that the gangrene attacked the stump, and this time it was an acute form and rapidly spread upwards. The man became exceedingly ill, sick, and delirious, and in this condition I amputated again high up in the thigh and with the most happy results. All the septicæmic symptoms at once subsided, and the stump subsequently healed perfectly.

It occurs to me as very possible that in this case the occlusion of the artery may have been the unsuspected cause of spreading gangrene which took place soon after the frost-bite. My next case seems to be an important item of evidence in this direction.

A man, æt. about 65, received a slight burn in the left hand. The result was spreading gangrene of the fingers, for which he came into the London Hospital. A large part of the hand was then black and sphacelated. We found that he had no pulse at the wrist and that his brachial artery was a solid thick cord. In the axillary, pulsation could be felt. During the next ten days the occlusion extended higher up and involved the axillary. The old man was very ill and clearly about to die if not relieved. I amputated just below the shoulder-joint and found the main artery completely plugged by a dense firm clot. The smaller branches were, however, pervious and bled freely. We had some difficulty with the wound, the edges of which sloughed a little. Ultimately, however, he recovered perfectly. In this case the arteries were not calcified, but very greatly enlarged and thickened. In the opposite limb the brachial artery was visible through the skin as a sinuous pulsating cord down the upper arm and in front of the elbow.

Encouraged by the result in this case, and about the same time, I amputated above the knee for an old gentleman living in Camberwell, but unsuccessfully. He sank within a week with gangrene of the stump. In this

instance the operation was done under most unfavorable circumstances and as a last chance. The patient was stout, in bad health, and the gangrene extensive and advancing. It was of the moist form, and the conditions were so urgent that I did the operation within a day or two of my first seeing the patient.

My next case was one of an ordinary form of senile gangrene of the right foot in a thin old man, *æt.* 72, in the London Hospital. He suffered also from prostatic retention of urine. Amputation above the knee was followed by good results so far as relief of pain and almost complete healing of the stump were concerned. The patient lived about five months afterwards, but never left his bed. He died ultimately from exhaustion by various causes, and with a gangrenous patch on the other foot. His arteries were very extensively calcareous.

In a fifth case I amputated for spreading senile gangrene of the left foot in an old woman, aged 75. She was suffering severely from the pain and irritation; was almost constantly delirious, and certainly not likely to live more than a week when the operation was done. She was, however, fortunately very thin. After the amputation she did well without an interruption. Her stump healed without a drop of pus and became perfectly sound. Her circulation was so feeble that we had great difficulty in preventing gangrene in the toes of the other foot, and had to keep it wrapped constantly in cotton wool. She returned home about a month after the amputation in tolerable health, but with the other foot still in a condition to cause great anxiety. I had advised her to have that limb removed also, as the best means of making the conclusion of her life comfortable and enabling her to leave her bed. It seemed certain that the toes would at once pass into gangrene if she were to sit up. My advice on this point was, however, declined by her friends. The removal of both lower extremities seemed to them a mutilation too formidable to be considered, and they took her out of the hospital.

The last case which I shall mention is one of unusual interest. The gangrene was senile in the sense that it was caused by calcareous arteries, but the patient was not advanced in years. He was much out of health from other causes, and the gangrenous process was preceded and attended by inflammatory action.

I attended Mr. H— in consultation with Mr. Linton Brunton, of Limehouse, whose relative he was. Mr. H— was only forty-eight, but he had worked hard and lived freely, and although not showing other signs of old age his arterial system had become most extensively calcareous. His pulse at the wrist was most feeble, so much so that frequently for days together it was scarcely perceptible. He was liable to become blue in the face on exposure to cold air, and suffered much from cold extremities. He had once at a railway station fallen unconscious, probably from cardiac syncope, and once or twice after mental excitement had been so prostrate that he was only kept alive by the freest use of brandy. Such being the state of his circulation, he was exposed one snowy day in 1880 to cold, and came home with his feet much chilled. Bullæ formed on his left foot and were followed by gangrenous patches of the skin and much swelling. The bones seemed to suffer more extensively than the skin, and abscesses subsequently formed about them. This might perhaps be explained to some extent by the fact that there was always a certain quantity of sugar in his urine. During the two months that I watched the case Mr. Brunton and I removed piecemeal many of the digital phalanges. Gradually the disease spread up the foot; abscesses with gangrenous patches of skin formed, and the ankle-joint became affected.

Cecily

We obtained the advantage of a consultation with Dr. Mahomed on December 24th, 1880, concerning the state of the heart, &c. Dr. Mahomed used the sphygmograph and gave me a detailed written report. It was to the effect that there was no proof of arterial disease in the upper extremities, nor any of valvular disease

of the heart ; the latter organ was believed to be feeble, possibly dilated, and probably fatty. The pulse was described as "extremely small, non-persistent, readily compressed, short ; artery not thickened." The urine was found to contain a large amount of sugar and also a considerable quantity of albumen (about one sixteenth). It had a specific gravity of 1033. Mr. Linton Brunton had often before demonstrated the presence of sugar, but we had never found albumen, nor was it found subsequently.

Mr. H— had been most of this time in a condition of very urgent illness ; often vomiting everything for days together, and not in the least expected to live. Towards the end of February his general condition was a little better than it had been, but as the foot got worse and worse we decided to amputate at all hazards. This was done above the knee on February 27th, 1881. The elastic bandage and strap were used, and Lister's precautions fully carried out. When adapting the flaps the femoral artery was observed to project between them like a small bone, and I was obliged to forcibly bend it back. It had been cut in the flaps much longer than the bone, and I feared that we should have trouble with it during the healing of the stump. None such, however, occurred ; both the bone and it proved to be sufficiently covered, and the healing was immediate and without suppuration. It is now about three years since this amputation was performed, and the patient has remained well, wears an artificial limb, and gets about almost as well as formerly. His diabetes has for some time disappeared. He has lived more carefully, but has been much troubled by ulcerations on the other foot.

These cases do not include quite the whole of my experience respecting these operations. In a case under one of my colleagues in which, about six months ago, I had pleasure in giving my vote in favour of amputation, the result was, I believe, most satisfactory, and my friends have informed me of several others.

The facts adduced, I think, justify the belief that

amputation through the lower third of the thigh is not a very dangerous operation even in advanced years and with most extensive calcareous degeneration of the arteries. I have never in a single instance had any trouble with secondary hæmorrhage nor encountered any difficulty in securing the vessels at the time of operation. I have always tied with stout catgut or carbolised silk, and have taken up the tissues surrounding the artery freely. The smaller vessels have been twisted, but I have never trusted to this for the main trunk. If it be admitted that the operation is not in itself dangerous, then I think there can be no hesitation in believing that it offers a most acceptable alternative to the miseries of death from slowly advancing gangrene.

ON A CASE
OF VERY LARGE
LYMPHO-SARCOMATOUS TUMOUR OF
THE TONGUE.

BY

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Read June 9th. 1885.

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1885.

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LYMPHO-SARCOMATOUS TUMOUR OF
THE TONGUE.

BY

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Received April 14th—Read June 9th, 1885.

THE case which forms the basis of my present communication was under my care about four years ago. The entire tongue of a young man, only twenty-two, was then removed on account of a tumour which had been slowly growing for more than half his life, and which had at length attained such a size that it impeded swallowing and even threatened suffocation. I have purposely allowed a considerable period to elapse before submitting the case to the consideration of the Society, because I wished to have the evidence afforded by time as to the true character of the growth. Two years had passed by, and as the patient remained quite well, I had begun to hope that we might assume it as probable that the tumour was of an innocent character. I am sorry to say, however, that after this long interval these hopes have been disappointed.

The whole mass weighed after removal seven ounces. It was by far the largest tumour of the tongue that I have ever seen removed, and I believe the largest on record.

The subject of the case was a medical student, aged, at the time of the operation, twenty-two years. His father was a medical man, and the conditions had, consequently, been carefully watched from the first. No growth had been observed, and no morbid condition of the organ noticed, during the first few years of life, and his father having had occasion to make applications to the tonsils in childhood, felt sure that he should have seen it, had anything been present.

The first symptom that was observed was at about the age of ten, and consisted of a swelling in the left side of the organ, with a rough papillary growth on its posterior surface. Without causing any inconvenience except from its size, the tumour continued to increase, and when, in the latter part of 1880, I first saw the patient, the organ was fixed in the mouth by its mere bulk, and speech was difficult. At this time the tumour consisted of a large rounded mass, deeply embedded in the tongue, and wedging itself against the sides of the lower jaw. The tip of the tongue was free, and could be moved on the surface of the tumour, but with this exception the whole of the organ was involved. The tumour could easily be felt externally as a hard mass bulging downwards behind the chin. The mucous membrane of the tongue over the tumour was quite healthy, with the exception of the posterior two thirds on the left side, which presented a coarse papillary growth, not in the least painful, nor ulcerated, but which was continuous with the substance of the tumour beneath. The surface at this part was nodular, like the outside of a mulberry. Had it not been for this growth, which implicated the overlying parts, so moveable was the mucous membrane and the superficial layer of muscular structure on the tumour, that one might have been tempted to hope that it was encapsuled, and might possibly be shelled out.

I had had the advantage of the opinion of Sir James Paget, to whose kindness indeed I had been indebted for having the patient placed under my charge, and I subsequently obtained that of Mr. Savory and several other professional friends.

It was felt by all that the time must come when it would be absolutely necessary to operate, and, encouraged by the long duration of the case, we were all hopeful that the disease was not in any sense malignant. As to its precise character, however, no one ventured a confident opinion. The operation was not performed until nearly a year after I had first seen the patient, and in the interval the growth had increased considerably, and the inconvenience had finally become unbearable.

The operation was done on November 20th, 1881, and I had in it the able assistance of Mr. Waren Tay and Mr. R. W. Parker.

The patient having been put under ether a preliminary tracheotomy was performed. It would have been better to have done this without the anæsthetic, for as soon as insensibility commenced the mass fell back upon the glottis and the patient was in the utmost danger from asphyxia. It was impracticable to draw it forward sufficiently to effect relief, and, after a hurried completion of the tracheotomy, we were obliged to do artificial respiration for some time to restore animation. The patient having well rallied the pharynx was next plugged with sponge, and an exploratory incision was made into the substance of the tumour in order to ascertain whether or not it was encapsuled. Finding that it was not so, that it was very firmly fixed on all sides, and that it was quite impossible to get the finger either beneath or beyond it from the mouth, I at once made an incision through the lip and chin and cut the symphysis of the jaw. Having freed the muscular attachments and widely separated the two halves of the jaw, access was gained to the base of the organ. Having well isolated the mass, partly by scissors and partly by tearing, the wire *écraseur* was

applied over its base just in front of the epiglottis, and the removal completed. There was no trouble from hæmorrhage.

No difficulty occurred in the after treatment. The jaw united well and a rapid recovery followed.

The drawings (Plate V) give a good idea of the appearance of the growth, both on its surface and in section. It will be seen that the mammillated patch occupies an area larger than a crown-piece, and that its growths are at least a quarter of an inch in height. The tumour itself, as seen in section, was almost globular, and measured two and a half inches across. It completely replaced the substance of the tongue, with the exception of a portion about three fourths of an inch in length at its tip. The posterior two thirds of the growth showed a greyish fibrous structure, which was divided into loculi by white bands. The anterior part was red, not so hard, and much more vascular; but obscurely divided in like manner into lobes. On its surface the growth was everywhere bounded by a tolerably well-marked layer of fibrous tissue. On its surface in many parts, especially in front, were seen what looked like the cut ends of small muscular bundles. There were no cysts, and nothing that was conclusively erectile. The muscular bundles of the front part were inseparably attached to the growth.

One half of the tumour was sent to the College of Surgeons, where it was examined by Mr. Eve, the pathological curator, who also obtained the assistance of Dr. Klein. The other half was examined independently by my eldest son, who had the valuable assistance of Mr. Rickman Godlee in inspecting the microscopic sections. These observers all agreed in reporting that the growth was of the connective-tissue type, and probably a round-celled or lympho-sarcoma. Here and there in its substance muscular fibres, atrophied by pressure, could be demonstrated. A tendency to the development of well-formed fibrous tissue was a strongly characterised feature. The enclosed loculi were of very various sizes and contained cells, which had rela-

tively large nuclei and an ill-defined cell substance. Between the cells a fine reticular tissue was seen in parts.

The much enlarged papillæ were infiltrated with cells, not apparently differing from those in the alveoli beneath.

Between the papillæ in some of the fissures the deep epithelial cells had much enlarged and undergone granular degeneration. Everywhere there was a sharp line of separation between epithelium and corium. (Pl. VII.)

It will be well before attempting to discuss the clinical relationships of this case to give its termination, and also to cite any evidence which can be obtained from the records of other cases more or less resembling it.

After his recovery my patient, who was a gentleman of great pluck, at once returned to his medical studies at Belfast. From time to time I heard of his being quite well, and two years after the operation I heard that he was intending to present himself for examination at our London College, and that I should then have an opportunity of seeing him and hearing how well he could make himself understood. About six months later, however, I had the disappointment of being informed of his death. A growth had very rapidly sprouted up in the floor of his mouth and had in a short time brought about the fatal event, partly by pressure and partly by hæmorrhage. I did not hear of the recurrence until after his death, and there was no post-mortem. I believe that there was no evidence of gland disease.

I am not able to produce either from the records of other surgeons or from my own experience, any case which is an exact parallel to the one which has been described. The following cases, however, in a fragmentary way, some from one aspect, and some from another, seem to throw light upon its probable nature. We may note that we have to deal with a tumour which invaded the substance of the organ in which it was placed; which

began in very early life ; which was attended by coarse mammillation of the surface ; which continued to grow painlessly and very slowly through a long series of years ; which never produced gland disease ; which recurred locally after removal, and which under the microscope presented the characters of an infiltrating lympho-sarcoma, with dilatation of blood-vessels.

The first case to which I shall refer is that of a child, aged three, in whom a mammillated growth on the posterior part of the tongue had been present from birth. It had caused no inconvenience but was slowly increasing. I excised it by means of the cautery, and the child, I believe, remains well. It did not go deeply into the substance of the tongue.¹ The method of removal destroyed the specimen for microscopic purposes.

Specimen 1067A in the Museum of the College of Surgeons is the tongue of an infant in whom similar conditions, but on a much larger scale, were present. It was shown by Dr. Hickman, in 1869, at the Pathological Society, and is described in vol. xx of the 'Transactions.' The infant was born with a mammillated tumour on the left side of the dorsum of its tongue as big as a plum stone. It was sessile and extended from the line of the circumvallate papillæ back to the epiglottis. Owing to its peculiar position, rather than its size, it caused death by suffocation within sixteen hours of birth. A committee appointed by the Society reported that it consisted of hypertrophied gland follicles and ducts with an intervening matrix of nucleated connective tissue. Its blood-vessels were large and numerous, but there was no definite erectile tissue.

Mr. Liston has recorded a case in which he operated on a lad, aged nineteen, for a tumour of the tongue which had been present from birth. In some respects this case resembles the ordinary macroglossia of young children, but in others it differs from it, and is suggestive rather of

¹ I possess a coloured sketch illustrating this case. It was produced at the Society's meeting.

a nævoid mole. The tongue filled the mouth and projected over the lips. It was elastic and compressible and its surface was crossed by large venous trunks. The papillæ over it were much enlarged, and granular points were numerous. There was an ulcerated fissure near its centre which frequently bled. Mr. Liston tied the lingual arteries and then ligatured the whole tongue. Unfortunately the patient died of pyæmia.

My next case (Plate VI) is clearly very like Mr. Liston's excepting as to size. Its subject was, when I first saw him in 1872, a lad of twelve. He is now a porter at Woking Junction aged twenty-four, and in good health. The state of his tongue has not materially changed. The portrait shows considerable hypertrophic enlargement of the organ chiefly of its left half, and the surface is covered with mammillated and granular points, just such as Mr. Liston described, and such as were present in the case which is the chief subject of this paper. Some of them were vascular, like parts of a venous nævus. The condition had been noticed soon after birth and it increased somewhat in growth. He was sent to the London Hospital from Farnham when he was twelve, because it had increased somewhat, and with a view to operation. I kept him under observation a few weeks and finding that the condition caused the boy no inconvenience whatever I deferred any interference. My decision has been justified by the fact that he has got no worse. This case seems to differ from that which my paper chiefly concerns in that there is no tendency to continued growth in the substance of the organ. It is very possible, however, that this tendency may yet declare itself.¹

The next case was a child under the care of Mr. Waren Tay in the London Hospital. The tumour was congenital and deeply placed in the substance of the left half of the

¹ The man who is the subject of this case attended the meeting for inspection. Further examination made it certain that although, as may be seen in Plate VI, the growth appears to involve both halves, it is really limited to the left, and only encroaches on the right by bulging over.

tongue, bulging both below and on the dorsum. The tip of the tongue was free, there was no papillary hypertrophy, and the tumour, although partly solid, consisted chiefly of cysts. I believe the child died after an operation for its removal.

Closely parallel to this, and presenting also features of similarity to my own, is a case which I find narrated by my friend Dr. George Brady, of Sunderland, in the 'Medical Times and Gazette' for 1867. An infant had, at birth, a bluish looking tumour under and in its tongue, which was taken for a cyst. It was punctured freely, and almost fatal bleeding followed. At the age of fourteen Dr. Brady attempted the removal of the tumour, which was then a softish solid. It was sought to excise the growth from under the tongue without removal of the latter, but the tumour was so ill-defined and the bleeding was such that the operation was not perfectly completed. The boy, however, recovered and at the end of several years was still well and without any new growth.

A case which has been recorded, in excellent detail, by Mr. Henry Arnott, in the 'Pathological Transactions' (vol. xxiii), well illustrates the combination of congenital hypertrophy of various different structures in the same organ. The infant was fourteen months old when he was operated on by Mr. Simon in St. Thomas's Hospital. The tongue was of very great size and lolled out between the stretched lips. It had been large from birth and had hindered sucking. The child died of pneumonia three weeks after removal, by the *écraseur*, of the greater part of the tongue. Mr. Arnott records that "there were present, first, a *nævroid* affection of the blood-vessels; secondly, a thickening and induration caused by a long-continued sub-inflammatory state; and thirdly, a general enlargement of the lymphatics. Rather large irregular spaces, with very thin walls, and mostly with no visible contents, were met with in every section. Some of these spaces were filled with blood, but for the most part they contained only clear fluid with a few vesicular bodies—swollen

epithelial cells—in contact with their walls, possibly dilated lymphatics.”

In none of the cases which I have quoted, excepting my own, was there any proof of the presence of any solid growth which might deserve the name of lympho-sarcoma. In Mr. Brady's case the growth was solid and very possibly of this nature; but it was not so diagnosed, and in Mr. Liston's we have no microscopic examination at all. In several of the others we must remember that the child died after operation in early life and thus no opportunity was afforded for the further developments which took place in mine. The only recorded example of sarcoma, so diagnosed, which I have been able to find, is one published by Professor Jacobi, of New York, and quoted, with valuable criticism and additional facts, by Mr. Butlin in his 'College Lectures.' In this instance the tumour was the size of a hazel-nut at birth, and as big as a walnut when, at three months old, it was removed. It was a firm, rounded, elastic mass, deeply furrowed on its surface. Its section was uniform, excepting that in its centre was a small cyst. Its external portions contained muscular tissue, but its chief structure was that of a spindle-celled sarcoma. There were some round-cells, but not much intercellular substance. The child in this instance was well five months after the operation but nothing more is known of it.

The last case which I shall quote is one published by Mr. Folker, of Hanley. In it a large solid tumour which had been growing for twenty years was successfully removed from the tongue of an adult man. It was diagnosed as “fibrous.”

The cases which I have adduced justify, I think, the conclusion that my case ought to rank as one of a group in which hypertrophic structures, present in the tongue at the time of birth, subsequently take on a more or less erratic development. These congenital excesses are probably analogous to moles and nævi of the skin; and in the tongue, as proved in several cases, dilatation of blood-

vessels and of lymphatics is a conspicuous element in the growth. We know that moles of the skin often show vascular as well as papillary and fibrous hypertrophy. All the structures, in irregular proportions in different cases, are involved much as we have noted in the series which I have this evening produced. Moles and nævoid moles of the skin sometimes take on growth tendency, and this may happen at any period of life, and sometimes, as is well known, melanotic sarcoma may occur in them. In some rare instances fibroid hypertrophy in moles shows a definite tendency to recur. I once removed for a young lady a huge pigmented and papillary mole which grew on the pubes and involved the labium. It was impossible to remove the whole. A tendency to subsequent growth was most definite, and second and third operations were required. The subcutaneous fibroid hypertrophy produced a dense hard mass an inch and a half in thickness. The pathological processes displayed in this case were probably very similar to those which occurred in the tongue in the case which I have had the honour to bring before the Society.

In conclusion a few words may be said as to the surgical aspects of the case. It might, perhaps, have been better, as regards the prospects of immunity, if I had operated when the patient first came under my notice. My reasons for delay were, that the growth had already attained such a size that the operation must of necessity be a very formidable one, and that as it seemed highly probable that the tumour was innocent so it was wise to put off the danger as long as possible. Guided by our experience in this instance, I should certainly in any similar case—in which a tumour in the tongue, however apparently innocent, was steadily growing, however slowly—be inclined to advise an early removal. It may be doubted, however, whether a patient in whom such a growth caused neither pain nor inconvenience and had been present for years, would be inclined to submit early to an operation involving the loss of the entire tongue. Such

would have been the conditions under which any surgeon who saw this case before I did would have had to advise.

(For report of the discussion on this paper, see 'Proceedings of the Royal Medical and Chirurgical Society,' New Series, vol. i, p. 458.

DESCRIPTION OF PLATES V, VI, AND VII.

(On a Case of very large Lympho-sarcomatous Tumour of the
Tongue, by JONATHAN HUTCHINSON, F.R.S.)

PLATE V.

Case of Lympho-sarcomatous Tumour (see p. 311).

FIG. 1.—The tongue, after removal, seen from before.

FIG. 2.—The same seen from the side.

FIG. 3.—A longitudinal section of the tongue from tip to base.

PLATE VI.

Case of Congenital Unilateral Hypertrophy of Tongue (see p. 317).
(A papillary, lymphatic, and nævoid mole.)

PLATE VII.

Microscopical Sections from Tongue, figured in Plate V.
(Drawn by Mr. J. Hutchinson, jun.)

FIG. 1.—Section of the surface of the tongue made through part of two of the large “papillæ” which covered it. The epithelial layer was everywhere well defined, and here presents nothing abnormal; the subjacent layer is very vascular and somewhat infiltrated with lymphoid cells, whilst towards the lower part these show a tendency to be arranged in irregular alveoli.

a. Horny layer.

b. Rete mucosum.

c. Vessels.

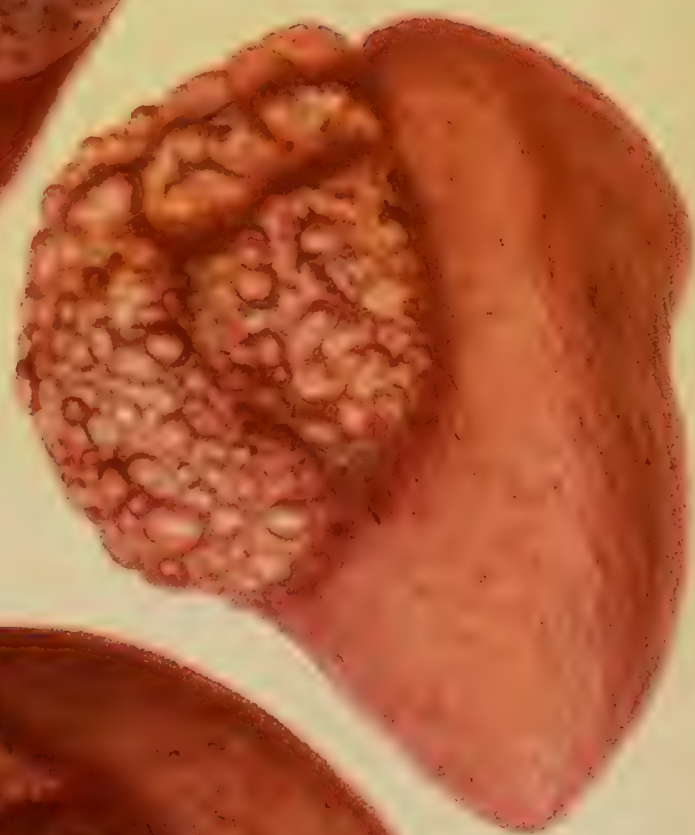
d. Deep part beginning to show an alveolar arrangement.

FIG. 2.—A section characteristic of the great mass of the tumour, showing large rounded alveoli filled with lymphoid cells of uniform size, enclosed by bands of fibrous tissue and intersected by finer strands of the latter.

Fig. 3.

Fig. 1.

Fig. 2.



E. E. Burgess del.

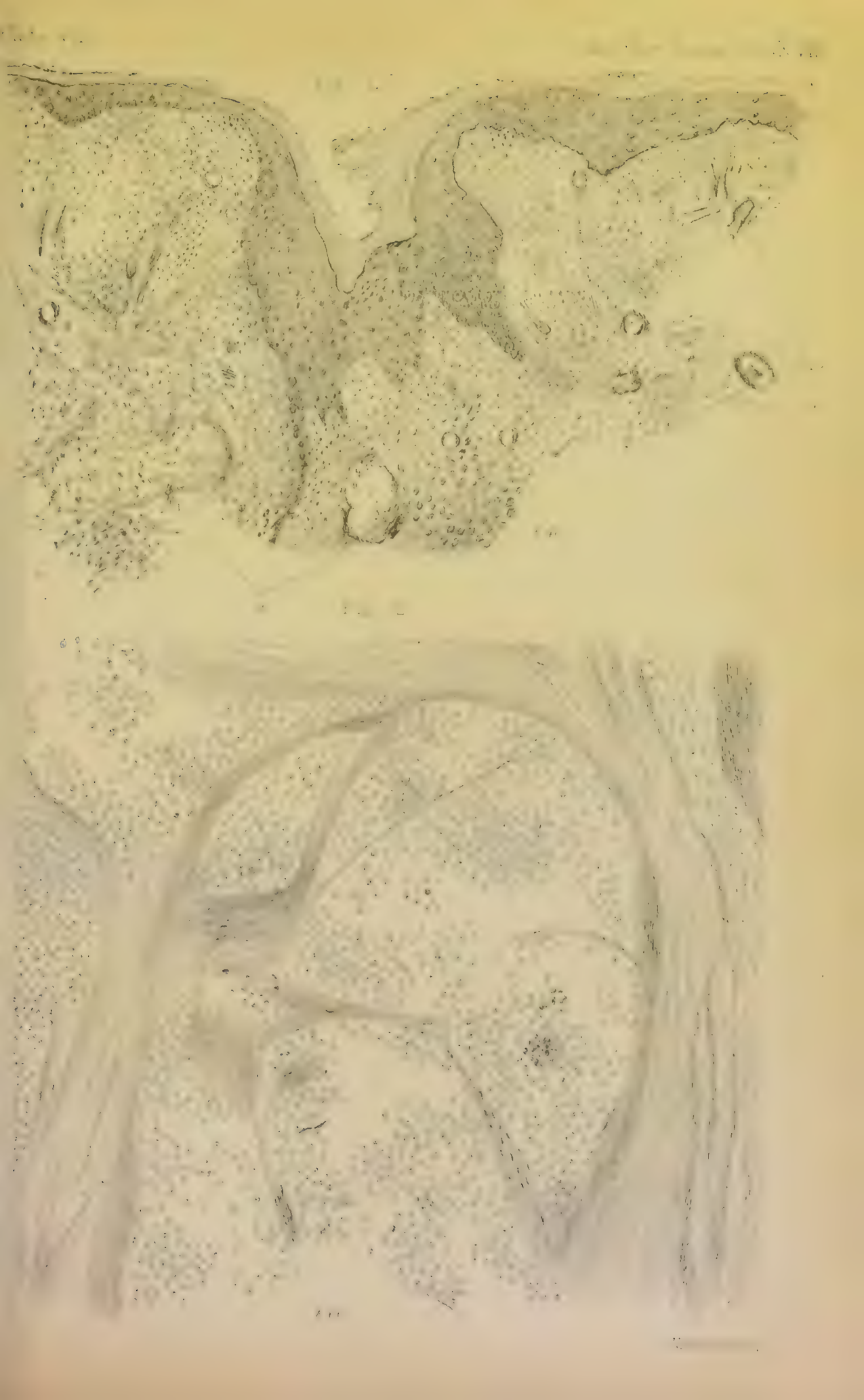
Lympho sarcoma of tongue

Martens Bros. Chromo. pub.





Congenital unilateral Hypertrophy of tongue.



A Case of Necrosis of the Lower Jaw from the Medicinal Use of Phosphorus. By JONATHAN HUTCHINSON, F.R.S. *Read March 12, 1886.*

MRS. S., a lady of about sixty-five, was sent to me in November, 1884, with extensive disease of the right lower jaw. There was great enlargement of the whole lower half of the face, and several sinuses opened externally. One of these was behind the ascending ramus and another larger one under the body of the jaw, not far behind the chin. In the mouth bare and discoloured bone was exposed along the whole length of the alveolus. The stench was abominable, and the discharge being profuse the patient was very much reduced in health. The bone, so far as I could ascertain, was quite fixed. In every feature the case resembled one of phosphorus-necrosis, and I have never at such an age seen necrosis of the jaw, of similar extent, in connection with any other cause. I questioned my patient as to whether she had ever lived near any factory where it was possible that she might have inhaled the fumes of phosphorus. She said, "No, but I have been taking phosphorus for the last two years." On further investigation this did not appear to be quite literally the fact for she had often left it off for a few weeks at a time. It remained true however, that, with but short intermissions, she had taken Kirby's pills, one three times a day, during this long period. They had, she said, "quite renovated her brain," and she spoke most gratefully of the physician, who had first recommended them. It is needless to say that he had not advised or sanctioned this prolonged use of them. She had got the prescription, and finding them beneficial, had continued their use.

When I first saw Mrs. S. she appeared to me too ill to bear the severe operation which would probably be necessary for removal of the bone. The diseased portion did not appear to be loose, and from experience of other cases I anticipated that it would be necessary to cut through the bone, with great risk that the process would afterwards be continued at the site of section. Phosphorus osteitis differs from all other forms in the slowness of its progress towards exfoliation. Beginning from the alveolus and extending outwards, the

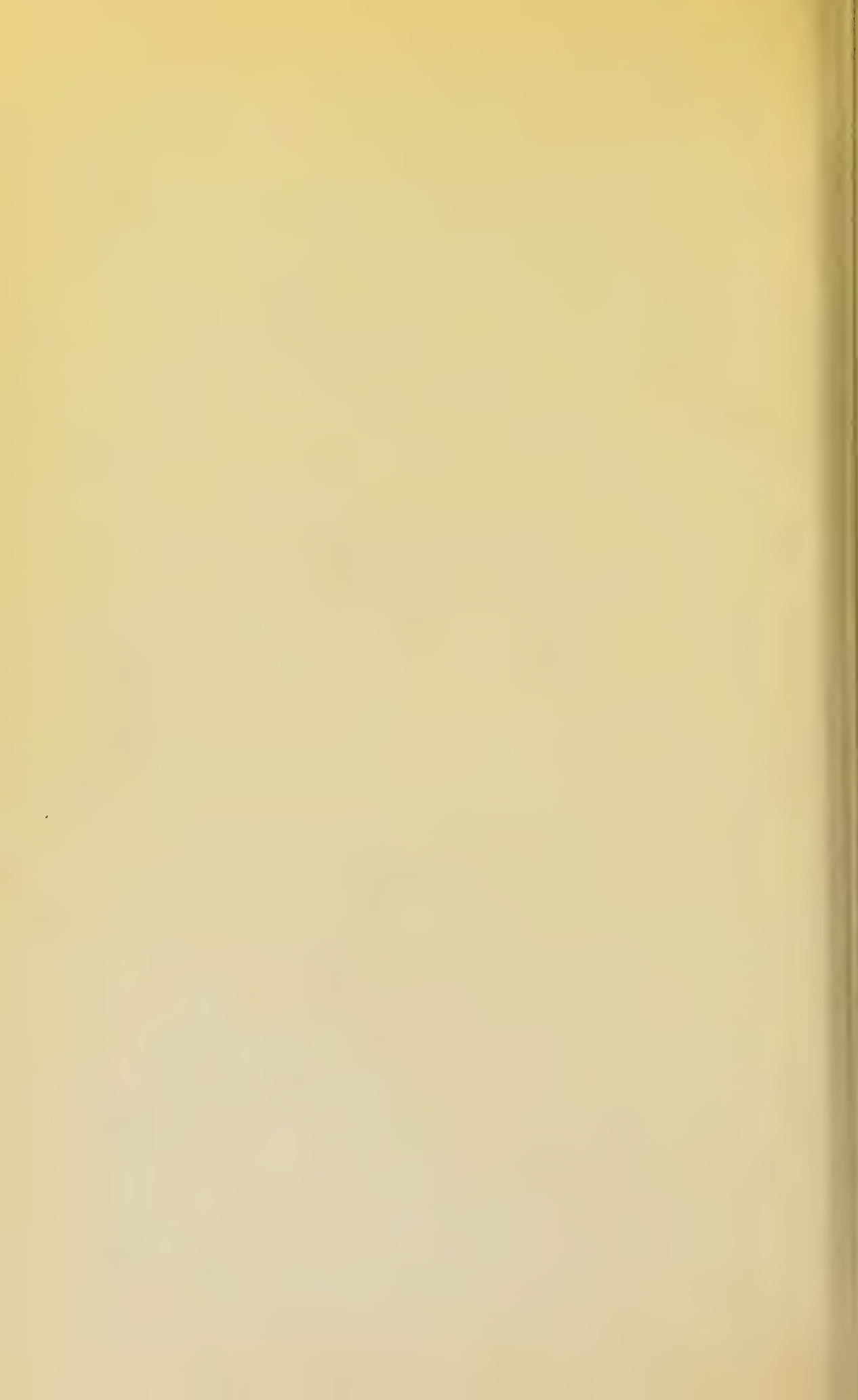
external surface does not wholly die, and the new shell is often united by continuity to the body which ought to be, but is not, wholly dead. A knowledge of this fact led me to fear a formidable operation and with no certainty of cure. I therefore advised that nothing radical should be done for the present and that Mrs. S. should try to improve her general health.

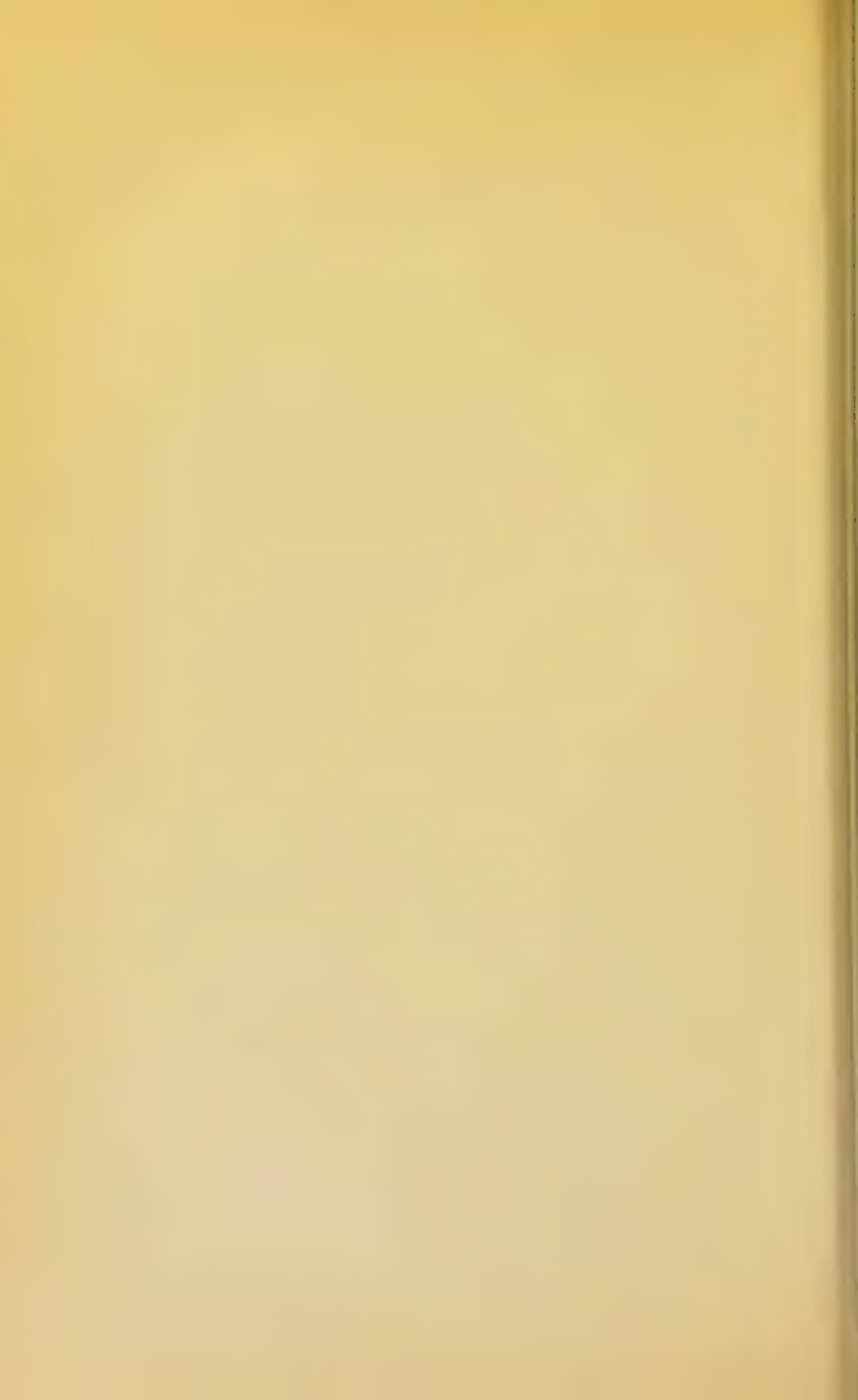
Four months later, in March of the present year, I saw Mrs. S. again. She was in much better health, and the general swelling about the jaw was somewhat less. Looking at her very deplorable state I now thought that it would be worth her while to run the risk of an operation. We had the advantage of a consultation with Sir James Paget, and the operation was decided on. When she was under the anæsthetic I again tried, as I had done before, to move the bone with a strong director, but again quite failed, and still believing that the diseased part was in continuity with the rest, I proceeded with a strong raspatory to detach the gum over the anterior part. Several teeth which were loose had been taken out, and I expected to cut through the bone near the chin. When, however, the bone was laid bare I found to my great pleasure that it could be made to move within the shell. After breaking the sequestrum I succeeded in extracting a large fragment without any necessity for cutting instruments. Other smaller portions were afterwards taken away, and then a clean cavity was left. The operation had been but a slight one, and Mrs. S. soon recovered. The portion removed measured about 4 inches in length, and had extended from the angle of the jaw to the first incisor. It comprised the entire thickness of the bone in its upper two thirds, but not its lowest part.

The question remains as to whether this necrosis really was the result of the phosphorus which had been taken. There did not seem to be much improbability in the supposition, for we know that the odour of this drug may often be easily recognised in the breath. It is even said that the breath may be made luminous by it. An intelligent patient once assured me that he had been startled on going into a dark room, soon after having swallowed a dose of phosphorus dissolved in oil, by observing that he exhaled luminous fumes. It is clear that such fumes might find their way into a carious tooth and induce osteitis just as it occurs in matchmakers. I once attended a child from York for necrosis of the lower jaw whose father had a match factory. The only possible expo-

sure that we could make out was that the nursery window opened over a workroom.

Kirby's phosphorus pills are in very extensive use, and this is the only instance that I have heard of in which jaw-disease could be traced to their employment. The patient had carious teeth at the time and inflammation of the jaw had begun in connection with one. It had been in progress for a year or more when I first saw her, and had thus presumably begun about six or nine months after the taking of the pills had commenced.





CONGENITAL ABSENCE OF HAIR AND MAMMARY GLANDS,

WITH

ATROPHIC CONDITION OF THE SKIN AND ITS APPENDAGES

IN

A BOY WHOSE MOTHER HAD BEEN ALMOST WHOLLY BALD
FROM ALOPECIA AREATA FROM THE AGE OF SIX.

BY

JONATHAN HUTCHINSON, F.R.S., LL.D.

Read May 11th, 1886.

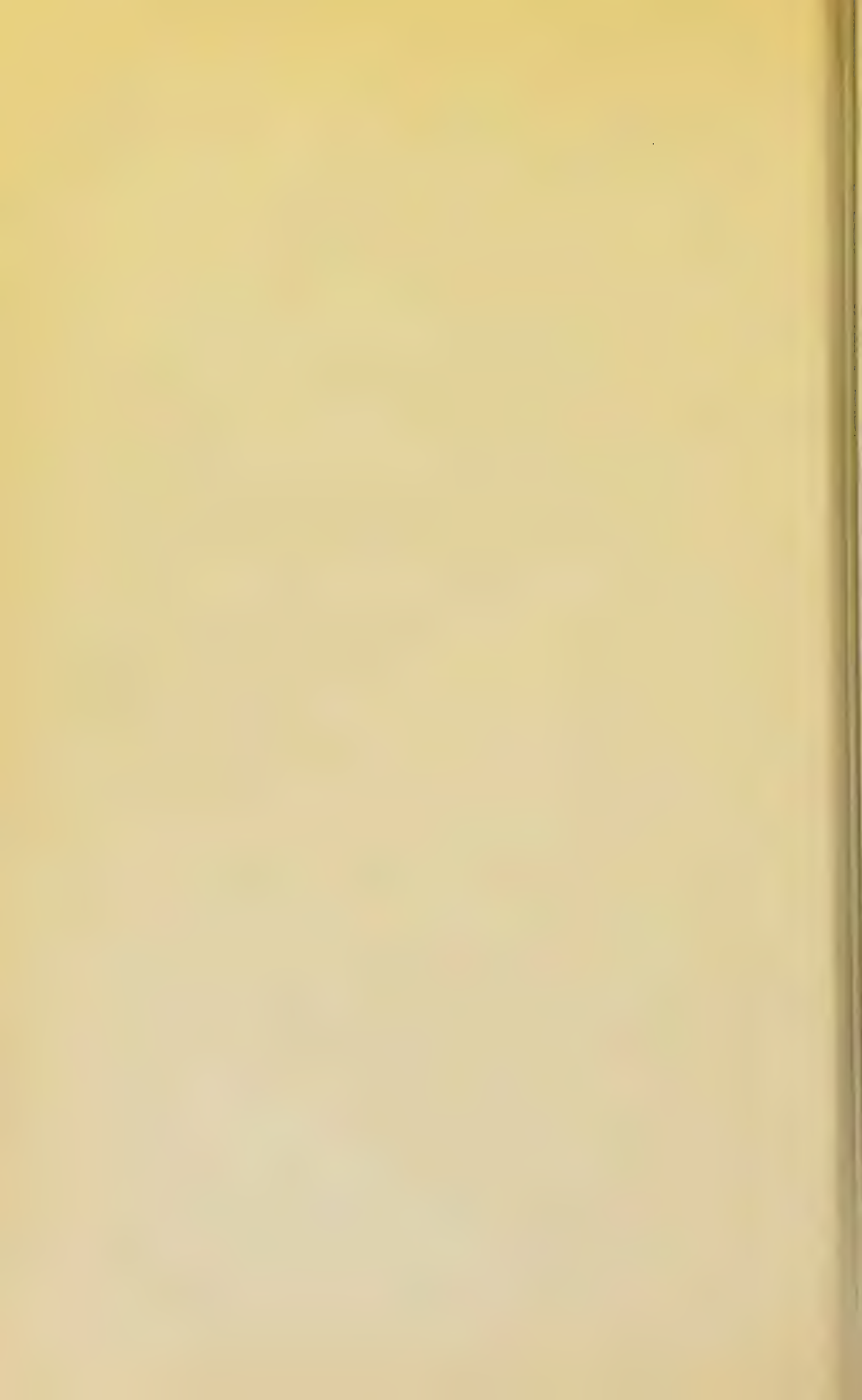
[*From Vol. LXIX of the 'Medico-Chirurgical Transactions,' published
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A BOY WHOSE MOTHER HAD BEEN ALMOST WHOLLY BALD
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BY

JONATHAN HUTCHINSON, F.R.S., LL.D.

Received January 12th—Read May 11th, 1886.

THE subject of this case, a boy æt. $3\frac{1}{2}$, presented a very peculiar withered or old-mannish look, all his features being thin and pinched. His fingers were shrivelled, and dusky, and their nails, which also were remarkably thin, were curved backwards so as to present more or less of hollow in the middle. His head was large and the anterior fontanelle not quite closed ; the scalp was exceedingly thin, and with the exception of a quantity of down, was quite bald. It looked semi-transparent and tight, and the veins coursing in it were everywhere conspicuous. The veins were probably larger than natural. A large trunk came down the forehead on each side of the eyebrow and communicated by a transverse branch at the

root of the nose. The inosculation across the middle line of the scalp were many. There was a peculiar blue tinge about the lips; it involved the skin and not the prolabium only. At first I thought that this was due to accidental staining; but after he had been half an hour in my room it much diminished, as did also the turgescence of the veins of his scalp. His lips were exceedingly thin. His teeth were all cut and were tolerably regular, but his incisors did not stand quite straight, most of them had some slight inclination into the mouth. On his shoulders he was so thin that his coracoids and the outlines of his acromion processes could be easily seen; the skin over them being not much thicker than brown paper. The tightness of skin was nowhere very conspicuous excepting on the scalp; thus, on the abdomen, arms, and thighs the integument was quite loose but everywhere very thin. His muscular development was slight in all parts excepting the thighs, which felt hard and had muscles quite out of proportion to the rest of his body (this remark does not apply to the buttocks). His genitals presented a very remarkable contrast to the rest of his body. The parts about the pubes and upper part of the scrotum were so full and plump that a suggestion occurred that he must have double hernia. This, however, was not borne out by examination, and I believe the simple fact was that the scrotum and adjacent parts of skin were in the state of those of a normally stout child, whilst everywhere else the skin, subcutaneous cellular tissue, and panniculus adiposus were almost absent. The true scrotum was small, naturally corrugated, and occupied only the lowest part of the genital pouch which I have described. I do not think that there was anything very unusual in this state in a child, but must admit that possibly there was some excess of subcutaneous development about the pubes and root of penis. His testes were well placed and of normal size. His penis, except that there was phimosis, was quite natural. His toes and their nails were in the same condition as his fingers. He did not walk quite perfectly,

always keeping his knees a little bent, but I could not make out any definite muscular defect. One other remarkable feature remains to be mentioned, he had no nipples and their sites were occupied by little patches of scar. These scars were exceedingly superficial and slightly marked, but I am sure that they were there. Nothing like a mammary gland could be traced.

The history which the mother gave me of the child was that he had had no ailments since his birth, was of cheerful disposition, and very intelligent. It had been necessary from cross presentation to turn during delivery, and for some days after birth he had been very blue, probably in a state of partial cyanosis. He was still liable to vary very much in blueness in connection with the temperature and states of excitement, but never now presented anything approaching a cyanotic condition.

I have now to relate the very extraordinary fact which is possibly explanatory of the singular condition of things just described. It will have been noticed that the chief defects present in the child were, an atrophic condition of the appendages of the skin and its accessory cellular tissue and fat, which became especially conspicuous in the absence of the scalp hair. With this we had a well-developed condition of the male sexual organs and an absence of the mammary glands and nipples. Now the mother of this child from the age of six to the present time had worn a wig on account of alopecia areata. At the age mentioned she began to lose her hair, which had previously been plentiful, in patches. She described the usual course of things, how the patches increased, and the whole scalp became smooth and bald, and how subsequently the eyebrows and eyelashes fell. After a considerable time her eyebrows and eyelashes grew again, and a few tufts of hair appeared on the scalp. But she had never regained her scalp hair sufficiently to dispense with her wig, and her eyebrows were still so poor that she was obliged to colour them. Excepting this alopecia she had no signs of deranged nutrition, being a florid, comely, well-developed

woman. The little boy was her first and only male child, but he had five sisters, all older than himself and all of whom had excellent development of hair.

Very curious speculations suggest themselves in connection with Darwin's theory of pangenesis. Under this hypothesis it may perhaps be possible that the germinal elements of the child's cutaneous system, and especially for his scalp, were derived from his mother, and were, in connection with her long baldness, very defective in vigour. With this would fit the entire absence of the mammary glands and their nipples; with this also would fit the normal development of the male genital organs and their skin, since he would be supposed to take these from his male parent. The fact that all his sisters had good development of scalp hair may be supposed to be explained by the suggestion that they inherited chiefly from their father.

It is to be added that the marriage was not one of consanguinity, and that no baldness or defects of development had been known in the family previously.

I may have perhaps a little over-stated the general absence of subcutaneous fat. Excepting on the head and hands, it was nowhere quite absent; and this remark especially applies to the abdomen and back. The deeply placed fat was less affected than the superficial. Thus, lumps of it could be detected at the root of the neck. The skin was everywhere destitute of natural elasticity and plump firmness, and where not dusky had an earthy pallor. The eyelashes were present but very weak. The eyebrows almost entirely absent.

I was indebted to Dr. Jago, of Mulgrave Place, Plymouth, for the opportunity of seeing this child and for some facts as to its history.

Remarks.—I prefer, for the present at least, to leave the above remarkable case without attempting to contrast it with other examples of congenital alopecia on record. From all these it differs, so far as I am aware, in the fact that the female sex organs (the mammæ) were absent, whilst the skin of the male sex organs was the only part

of the integument in a normal condition. These peculiarities become of the greatest possible interest when we remember that he appeared to inherit his defect from his mother. I am well aware that the explanation hinted at is a mere conjecture, and that there are a multitude of facts which might seem to militate against it. We cannot afford, however, in investigating the very difficult subject of hereditary transmission, to neglect any hint which the facts of pathology may offer. I need scarcely say anything as to the well-known law that defects, the result of disease or injury occurring in the parent and not congenital, are not transmitted to offspring. Everyone knows that circumcised fathers beget children in whom the prepuce shows no modification. To this law the case I have recorded seems to offer an exception, for there was not the slightest doubt that the mother's loss of hair was caused by the common form of alopecia areata, and did not begin till she was six years old. In fact, her hair grew again several times after its first falling, and again came off. Some will probably be inclined to consider that the mother's condition and that of her only son were associated as a mere coincidence and that the one was in no way dependent on the other. It is indeed precisely because this connection seems so probable, whilst it is in flat contradiction to received opinions, that I have thought the case worthy the attention of the Society.

(For report of the discussion on this paper, see 'Proceedings of the Royal Medical and Chirurgical Society,' New Series, vol. ii, p. 116.)





PRESIDENT'S VALEDICTORY ADDRESS

AT THE

SIXTH ANNUAL GENERAL MEETING, JULY 2ND, 1886.

By JONATHAN HUTCHINSON, F.R.S.

GENTLEMEN,—This is the last occasion on which I shall sit here as your President, and I shall ask your permission to make a few parting remarks. In the first place let me congratulate you on the election of my successor, which you have just completed. There is not within the British profession a more accomplished ophthalmic surgeon than Mr. Hulke, nor one in whose hands the interests of the Society will be more safe. Next, I have a message to deliver to you from our former President, Sir William Bowman, an expression from a sick bed of his best wishes to all his colleagues in the Society. Many present will know that Sir William has recently been laid up by a severe and painful illness (*sciatica*), and will be most glad to be assured that he is now making steady progress towards recovery. That he has the hearty sympathy of us all I need not say.

In looking back over the three years during which you have permitted me to occupy this responsible post I am glad to believe that the Society has well maintained its former character and prosperity. Our volume of 'Transactions' has considerably increased in size and very much in the liberality with which it has been illustrated. Thanks to the noble gift by Sir William Bowman, our financial condition has been made secure, and we have been able to devote funds, not only to the production of

plates for our 'Transactions,' but to the purchase of books for our Library. When the vacation is over the Society will begin our seventh year without embarrassments and with every adaptation for successful work. All that will be wanted are steady, earnest, and able workers, and these I feel sure will be forthcoming.

The three years over which I look back have not, it is to be admitted, been characterised by any great events in the world of ophthalmology; neither at home nor abroad have any great discoveries been made. Possibly we have not at present any Graefe amongst us. Still I believe that we may claim to have made good progress towards accuracy in many departments of clinical and pathological knowledge, and much work has been accomplished in the true Graefe spirit.

The introduction of cocain as a local anæsthetic is perhaps the most obvious and important novelty of the day, and, respecting this drug our Society has been prompt, not only in spreading knowledge as to its virtues and mode of use, but also, thanks principally to the candour of Mr. Nettleship, as to some of its special dangers.

At the suggestion of Dr. McKeown, of Belfast, the Society made an effort two years ago in the department of preventive medicine. It is not often that an opportunity occurs to us of doing anything of this kind, and there can be no doubt that the steps which we then took were in the path of duty. Although we did not succeed with the Government officials quite to the extent desired, still I have no doubt that great good was accomplished by calling the attention, not only of the profession, but also of the public, to the large amount of blindness which results from purulent ophthalmia, and to the fact that much of it might be prevented. I have not the least doubt that, as the result of the Society's efforts, thanks to Dr. McKeown, there are now fewer blind children than there would otherwise have been and that the number will diminish year by year.

It was one of the aims of those who organised this

Society to secure the co-operation of others than ophthalmic surgeons; we wished to cultivate our department as a specialty, but at the same time as a branch of medicine in general. It was desired that physicians, surgeons, and family practitioners should find it worth their while to join us, assist in our discussions, and read our 'Transactions.' It may be freely acknowledged that there is much difficulty in realising this aim to the full. So many of the topics which interest us become special as soon as they are developed and pass beyond the scope of those not accustomed to the everyday use of technical expressions. The details of operative practice and of the adaptation of spectacles, both matters of the utmost importance to the specialist, are unprofitable to all others. I think, however, that the Society may claim to have endeavoured to keep on a broad foundation. Both the two Bowman Lectures were on general subjects, and one was given by a physician, whilst the only special discussion which we have had was on Graves's disease, a malady more medical than ophthalmic. Nor will anyone who glances through our volumes of 'Transactions' find reason to complain of want of general interest. We have tried to regard the eye as part of the body and its diseases as part of general pathology, and not, I venture to think, without some success. If ever the Ophthalmological Society of Great Britain should become one of pure specialism it will not be without a struggle to avoid the fate.

The discussion on Graves's disease to which I have just referred was in some sense an experiment. Instead of having the subject introduced by an address, we issued to all members a series of printed questions and requested our members to come prepared to give their experience. It was an effort at the organised collecting of clinical facts rather than a discussion. We did not invite argument or the statement of opinion, but simply the production of well-observed facts. The meeting was, I think, successful, for we had brought before us a large body of evidence, much of which was novel and valuable. For

myself, I may say that my knowledge of the disease in question was much increased. I was especially impressed by the facts which seemed to show that a very large proportion of cases, if watched long enough, are found to recover. The disease in its tendency, if the patient survives, to spontaneous recovery, seems to afford us the best type that we have of a class of what may be considered functional or, at any rate, temporary derangements of health. In this respect it stands in strong contrast with such maladies as ataxy, bulbar paralysis, and the like, which, unless cured by treatment, tend gradually to get worse.

Dr. Wilks, I remember, told us that in his experience almost all cases ended eventually in recovery, and no one mentioned cases in which the malady lasted indefinitely, although all of course knew of fatal ones. Surely this capability for recovery is one of the most remarkable and distinctive features of the disease.

If I do not mistake it will be the interest of this and of similar societies in the future to depend less upon the voluntary contributions of individual members and to attempt more in the way of organised discussions. It is true that in the discussion which follows an original paper opportunity is afforded for all present to bring forward their quota of facts. This is not, however, exactly what is wanted. On such occasions men often speak from memory only, and many, for lack of memory, do not speak at all. But few have taken the trouble to make themselves acquainted beforehand with the author's views or to prepare their own. There is too much of the impromptu and slipshod. Now, if each of our societies had a small standing committee to arrange for the bringing forward from time to time of definite questions concerning some special disease respecting which a brief, clear exposition should be made in print and freely distributed a considerable time before the discussion, I believe that great good would be done. The interest of the members generally in the topics would be increased, the meeting would be better in-

formed beforehand, and the facts communicated to it would be such as had been previously well considered. It would be not difficult, I think, to settle in this way a number of questions which go on from year to year without being brought to any definite test. The cases which we call rare are only relatively so, and that there are any which are unique we may wholly disbelieve. All that is wanted is, to collect the experience of many observers and bring it to a focus. It should be the aim of a society like ours to play the part of a concentrating lens. This done we should find much that is now dark well lighted up.

Permit me to illustrate my argument by mentioning a few subjects appropriate for our own Society's work.

The opinion of authorities as regards the general fact as to tobacco amblyopia is, I believe, now fairly unanimous. The incredulity which existed twenty years ago at the time of the first discussion on a paper of my own at the Hunterian Society no longer exists. We are no longer told in triumph that the Turks smoke more than the English, and that amblyopia is unknown amongst the Turks. This settlement of opinion upon a subject of really great importance has come about as the result mainly of repeated discussions at our societies. There is room yet for still further examination of details and of cognate topics. Let me suggest the following topics: Does the amblyopia of smokers ever relapse when once recovered from? I have known many cases in which those who had recovered from severe attacks resumed the habit of smoking, and I have not met with a single one in which a return of the symptoms ensued. I never yet saw a second attack. It may be that those who have once been threatened never indulge again so freely as before, but however this may be I feel sure that there is at any rate no marked tendency to recurrence. It is very extraordinary that it should be so, and possibly I am mistaken as to the fact, and form my impression from a too limited sphere of experience. It is just one of the points which half an hour's debate in a society like this, if taken a year or so after attention had

been drawn to it, might settle at once. I should like also to ask whether it be not true that many smokers recover from amblyopia of the most definite kind without ever relinquishing the habit or even without much reducing it. Next, I should like to know what the recent experience of my colleagues has been as to neglected cases, and whether this form of amblyopia ever progresses to absolute blindness. In the early days of the investigation we made the diagnosis less accurately than now, and included some cases which possibly did not belong to the group.

In connection with tobacco-amaurosis we have that which is said to be produced by spirit-drinking. Have our members seen many cases of true uncomplicated amblyopia potatorum, and if so, what was the kind of spirit which produced the result? Most of the cases which have passed under this name have been, I feel convinced, really due to tobacco, the patient having been both a smoker as well as a drinker. In some there has been renal disease. I never myself saw a single case in which I could attribute the failure of sight to spirit-drinking alone.

A most interesting group of cases of great importance to the neurologist as well as to ourselves might be constituted of those in which women who do not smoke become the subjects of progressive failure of sight which ends in blindness without any other symptoms of nervous disorder. They are rare cases and could only be collected in any number by an appeal to a Society.

I find in one of our volumes of 'Transactions' a case recorded by Mr. Nettleship in which a very young child became blind without head symptoms, and after a while recovered its sight. From first to last no changes were visible in the eye. It would be of very great interest to medical science to ascertain whether others have observed similar cases, and if so, whether the circumstances under which they occurred afford any clue to the nature of the morbid condition which thus accomplishes the temporary extinction of a special sense. Is it possible at other

periods than childhood? Is it in any way allied to the temporary and usually much shorter failures of sight which sometimes occur as a part of migraine? I mention Mr. Nettleship's observation simply as an instance of a very singular and inexplicable occurrence which ought not to be allowed to be forgotten, but which ought to excite our curiosity and induce a systematic search for parallels. It would be easy to find many such amongst the invaluable records of rare cases which form such an important part of our volumes. In fact the whole subject of retro-ocular neuritis, as it has been assumed to be, in other words, of blindness without ocular changes, is well worth systematic investigation. Despite the introduction of the ophthalmoscope it still remains the fact that there are certain cases in which "the patient is blind and the surgeon can see nothing," and their explanation is as yet very difficult. We must have them carefully collected in as large a number as possible, and then systematically grouped according to their true affinities. Several valuable papers already exist, the one from which I have quoted amongst the number, which would help a committee to put the subject into proper shape for discussion, but none of them approach the completeness which, I trust, would be assumed after an evening's debate before this Society.

We ought, I think, to be ambitious for our 'Transactions' that they should become encyclopædic—that they should constitute a complete store for clinical reference in all that concerns the more recondite facts of ophthalmic science. It will need some systematic effort to make them such, and a small standing committee of energetic men could not, in my humble opinion, be better employed in the cause of ophthalmology than in looking up the topics which have claimed the attention of others and received none from ourselves; we ought, I think, no longer to trust entirely to the chance contributions of our members. It is now eight years since Mr. Morton published, in the 'Moorfields Hospital Journal,' a case in which a woman

in the secondary stage of syphilis suffered from acute and long-persisting interstitial inflammation of the cornea. So far as I know, that case remains practically unique. I have seen a few doubtful and ill-marked parallels, but never one which could properly be placed by its side. Why keratitis should be so common in inherited syphilis, and so rare in the acquired disease, is one of the pathological enigmas for which as yet we have no answer. But apart from its mere rarity there were present peculiarities in the kind of inflammation in Mr. Morton's case which were of extreme interest, its very protracted duration and severity, so different from what is usual in syphilitic inflammations when treated by specifics, must also claim our wonder. Is it probable that mercury and iodides cannot influence the cornea, or only in a limited degree? We know that they do not act quickly or definitely in the keratitis of inherited taint. Before we allow ourselves to form opinions upon these points we must try to get more facts. Mr. Morton's case was not sent specially for his delectation; there are others like it if we could but find them, and to find them we want the help of the army of trained observers now associated together in this Society. Our volumes contain, so far as I know, no reference whatever to the occurrence of keratitis in acquired syphilis. Surely it might be well to look into the matter, and either put on record more cases with a complete description of their peculiarities, or, failing that, to state prominently, and after due research, that they are not to be found.

Amongst our clearest duties in reference to the general profession is that of affording correct appreciations of the symptoms displayed by the eye as they may bear upon the recognition of disease and diathesis. The retinitis of albuminuria and of diabetes, the hæmorrhagic retinitis of gout, the Argyll-Robertson pupil in ataxy, the choked disc of meningitis and cerebral tumours, are all examples of what I refer to. So also is the occurrence of interstitial keratitis as a revelation of inherited syphilis; such also the discovery of atrophic patches in the periphery of the

choroid as implying syphilis, either acquired or inherited. Respecting all these and many others of which physicians and surgeons daily avail themselves for the diagnosis of other maladies, it is most desirable that we should spare no labour to avoid mistakes. Let me take the last mentioned and ask whether our information is sufficiently exact. Can we say that disseminate choroiditis always implies syphilitic taint, and if there are cases which do not acknowledge that cause, to what are we to attribute them, and how are we to differentiate them? Without the least doubt such patches are of very high value in the direction indicated, but whether they are trustworthy to the extent which I have found some of my distinguished medical friends inclined to rely upon them, must, I think, be doubted. I should like to see this clinical question proposed to the members of this Society: Have you in your experience met with good examples of disseminated choroiditis, in which there was good reason to doubt the previous occurrence of syphilis, and if so, under what conditions?

I am not without a fear that in thus venturing to make rather detailed suggestions for future work I may seem to be assuming the duties of an incoming President rather than modestly discharging those of one who is taking his leave. In this matter, however, I have confidence in your kindly judgment. I may also hint that I am after all only urging anew the same things which I suggested when I first took office. It is, I believe, no part of your President's office to signalise those who have most zealously contributed to the success of our meetings. To do so would be invidious and would risk offence. I am much tempted, however, to do the thing in another way, and to mention by name those who have contributed nothing. It is the practice of at least one of our artists' societies to mention each year, at the end of the catalogue of pictures, the names of those of their members who have not exhibited. Might we not imitate the custom with profit? The contributions which we make to medical

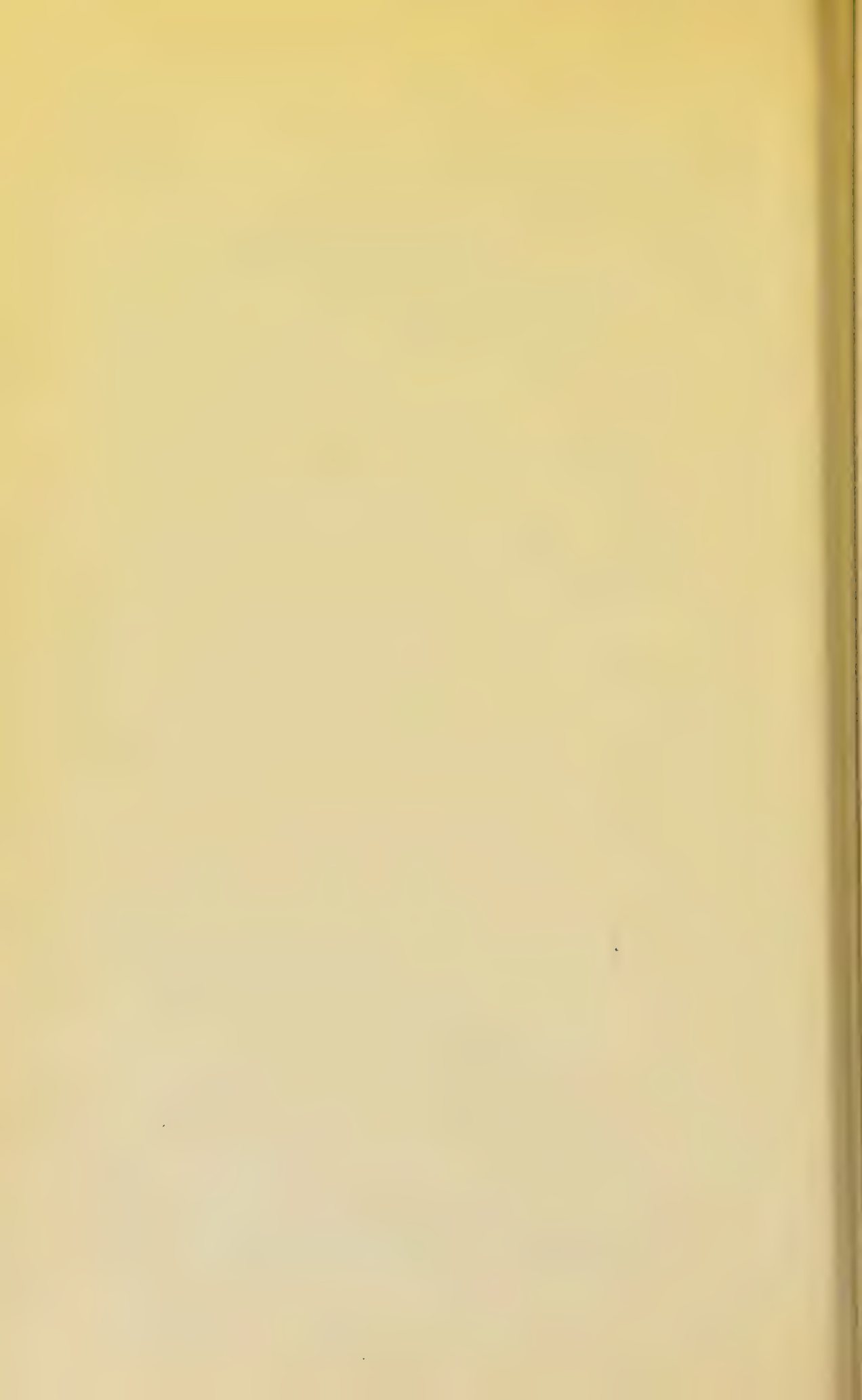
literature are in some sense our pictures, and no man should feel quite at his ease in his autumn holiday if he is conscious that he has painted nothing. You may think that I place a high value on medical literature and on the duty of writing papers. Indeed I do. I have not the slightest fear of over-production, and I certainly rank the duty of recording the experience which falls to our lot as amongst the most binding of those which attach to our profession. Only by such means can we advance our science and eliminate its errors.

It is my deep conviction on this point which has led me to urge that our Society should do its best to organise its work so as to secure the help of as large a number as possible. In all our large towns hospitals for diseases of the eye are being founded, and our aim should be that every one of these should be made useful to our Society. There is much, very much, yet to be done, and it is only by organised effort we can hope to accomplish it.

Before I conclude, it is fitting that I should say a few words on the mournful subject of the heavy losses by death which our Society has recently sustained. We have lost during the last year our Treasurer and no fewer than four who either were or had been Vice-Presidents. In Mr. Streatfeild I believe the public lost one of the most skilful of living operators, and a man accomplished in many departments. Mr. Wordsworth was a shrewd observer of disease, and in former years did excellent work. He was, I believe, next to Mackenzie, of Glasgow, the first in England to recognise tobacco amaurosis and to trust the evidence as to its cause. Of Mr. T. Shadford Walker, Mr. William Walker, and Mr. White Cooper, I need not say more than that each in his sphere practised his profession in a noble spirit and to the widespread benefit of his patients. These are all gone to their rest, and for us, whilst we feelingly regret their loss, the lesson is plain that we should work while for us it is day. It is not safe to delay the record of experience or the enunciation of thoughts, which, for anything we know, may prove of great

value to those who follow us. We must not forget to make our professional wills, and to bequeath, so far as it is capable of bequest, our scientific wealth.

Gentlemen, I have now only to say good-bye, and to assure you that the honour of having been the second President of the Ophthalmological Society of Great Britain will be one upon which I shall ever look back with pride.







On school ophthalmia.

By JONATHAN HUTCHINSON.

I SUSPECT that most of those present have of late had their attention drawn to the prevalence of a form of ophthalmia which leads to granular lids in a class of society hitherto almost wholly exempt. The cases have occurred chiefly, but not exclusively, in schools, and much anxiety on the part of school managers has naturally been excited. Not a few of us have probably been asked during the last few months to sign certificates of freedom from ophthalmia which were required before the pupil could return to school. The matter presents several features of much importance, in part social and in part medical; and although I must at the outset confess that I have nothing original to put forward, I have thought that a good purpose might be served by bringing the subject before this Society. I wish especially to afford opportunity for the contribution of experience by other members. It is not improbable that before many months are passed some of us may be called upon to advise school committees as to the measures which they should adopt, and it will be well that we should be prepared beforehand by an exchange of opinion on the subject.

I was myself in former years on several occasions asked to report on epidemics of this form of ophthalmia in workhouse schools, but not until quite recently have I heard of them in middle-class schools. During the last

few months facts have been brought to my knowledge respecting four or five well-managed schools in distant parts of the country which have suffered. For obvious reasons I shall not mention any names, and may perhaps be permitted to ask those who may speak in the discussion to observe reticence in this matter. The type of the disease, so far as I have seen it, appears to be very mild. Especially is the first stage, that of congestion, liable to be overlooked on account of its very slightly marked character. Not a few cases have escaped recognition until the lids were distinctly granular, and then the patient could not recollect that he had had more than just a day or two of irritability and redness.

At one school which I visited, at least 50 per cent. of the boys and several of the teachers and servants had suffered. The type had been very similar in all. A few days' slight soreness and redness of the eyes, which passed off almost without treatment, to be followed by a definite but mild condition of granular lids. Most of the eyes which I inspected showed only the papillary conditions, but a few had small sago grains. None of them showed advanced or severe conditions, but many had resisted more or less prolonged treatment. The disease had been present to a slight extent before a vacation, and on the reassembling of the school after it, several boys had returned uncured, and the disease had again rapidly spread. There was nothing in the least faulty in the hygienic arrangements of this establishment, and ophthalmia had never been known to prevail before.

In several instances there was a report that the disease had spread in the families of boys who had returned home infected, but I have not had an opportunity for personally investigating any of these. I have seen several uncured patients who had returned to their families, and from whom during a period of months no contagion was known to have occurred. In all these, however, treatment was being continued, and precautions as regards towels, &c., had been observed. It may be reasonably supposed that

the danger of contagion is not great so long as curative applications to the eyes are being used.

In the school to which I refer, 5 per cent. of the boys were recognised as being affected in September, 1893. In October of the same year there were 40 per cent., and in December no fewer than 50 per cent. In all the cases recognised boric acid lotions had been used, and for the whole school the utmost care as to lavatory and bedroom arrangements had been taken. With the exception of isolation, all reasonable precautions had been enforced. It must be added, however, that in no case had the upper eyelid been everted for treatment. In this instance many families resident in the town suffered, and they were chiefly those with whom pupils from the school had visited.

That this disease is exactly the same in the children of the rich as in those of the poor is well proved by reference to a report which I wrote in 1866 after an inspection of a workhouse school at Brentwood. The terms in which I recorded the facts in that instance might have been repeated in the case of the high-class school to which the remarks just made refer. The chief difference is, that in the case of the workhouse school the disease had prevailed for six years, whereas in the latter it had been recognised only six months. At Brentwood (a school of 310 children) I found about a third suffering from "ophthalmia of a chronic catarrhal form." In only a few had the lids passed into a badly granular form, and I did not consider that any presented the true trachoma bodies. The chief difference between this workhouse school and the one which I have recently visited would appear to be, that at the latter a granular condition of the lids was more easily assumed, and with much less preceding conjunctivitis. This, however, may possibly have been in part due to difference in observation, and to my having of late recognised as "slightly granular" conditions which I should formerly have passed as healthy. This last remark leads to another. It is that the diagnosis of "granular lids" in their early and slight stages is by no means easy.

After all, the papillæ which when enlarged constitute "the sandy lid," and give rise to the slightly villous appearance which constitutes the second stage of the disease, are normal structures, and large allowance must be made for varying conditions within the limits of health. It is easily possible to say of some lids they are not, and of others, that they are "granular" in a morbid sense, but between these are a host which no amount of experience will enable a man to speak positively about. I know of no sign which can be trusted in all cases, and during the prevalence of an epidemic the only safe rule is to treat all suspicious cases as if they were real ones. I need scarcely say that in the class of cases to which I refer no help is to be obtained from the absence of sago grains, for these are conditions met with only in late stages and pronounced cases.

If there is difficulty in diagnosis before treatment there is yet more afterwards, and nothing short of immunity from relapse for a month or two after cessation of treatment will justify a surgeon in pronouncing a case to be cured.

The disease to which, when I was on the Moorfields active staff, we used to exclusively apply the name "catarrhal ophthalmia" was a very peculiar one. It was an acute conjunctivitis, attended by great congestion and free mucous secretion. The ocular conjunctiva was always affected. The congestion had the peculiarity of being in patches, and so closely placed were the vessels that these patches often looked like ecchymoses. By the aid of a lens, however, you could always demonstrate that the patch consisted of dilated vessels, and that there was no real hæmorrhage. I have made this demonstration many a time. The two eyes were always affected together, and with almost equal severity. There was never any pain or intolerance of light, and the patient, with his red eyes, would look you full in the face. I do not know that I ever saw an isolated example of this complaint. Often two or three members of the same

family would come in company, or if not, there was almost always the history of others in the house having suffered. Yet, although thus most definitely contagious, I do not recollect to have been ever made acquainted with an epidemic which spread further than a few families. I never heard of it going through a school, or being prevalent in a town or village. Possibly one reason for this is that the disease is of very brief duration, and that whilst it lasts it is very conspicuous, and warns its subject to keep the house, and others to avoid him. Its attacks are usually over in a week or ten days, and although its disappearance is hastened by astringents it appears to be capable of spontaneous cure. It is not liable to recur, and, as a rule, leaves no sequelæ.

No schoolmaster in his senses would allow a child with this form of disease to sit with others. It is a malady which, although really trivial, looks alarming, whereas the reverse seems to be the case with the form of ophthalmia which leads to granulation.

Whether we think that the term "catarrhal" is appropriate to this disease will depend upon the meaning which we attach to it. There is little or no evidence as to its being induced by catching cold, and it is wholly distinct from the suffused eye which so frequently attends an ordinary coryza. That it spreads in families by contagion there cannot be the slightest doubt, and that when it does so the ordinary constitutional symptoms of "a cold" are absent is also undoubted.

That this form of conjunctivitis does not, as a rule, lead to granular lids is certain, but we must not hastily assume, that it may not sometimes do so in individuals predisposed by race or state of health. Nor can I wholly reject a suspicion, that these cases of so-called catarrhal ophthalmia are only very exaggerated forms of that which is the parent of granular lids. It may be the fact that the blotchy congestion of the ocular conjunctiva which is such a conspicuous feature of the disease occurs only in individuals of peculiar predisposition, and that most of the

cases do not show it. Against this supposition there is, however, as already hinted, the fact that in considerable epidemics not a single patient has ever exhibited any condition approaching it.

If the "pre-granulation ophthalmia" and the so-called "catarrhal" or "puro-mucous" ophthalmia be really distinct diseases, they have certainly some important features in common; both are very contagious and of very brief duration, in both the two eyes suffer together, and in both the same class of remedies are curative.

Mr. Nettleship, for reasons similar to those given above, has proposed to abandon the term "catarrhal," and gives an excellent description of this type of ophthalmia under the name of "muco-purulent."

Dr. Noyes,* at p. 293, has a paragraph which probably refers to the disease in question. He states that the attacks usually last from three to ten days, and that the disease may be wide-spread in a community. He does not, however, mention the conspicuous features to which I have adverted, and he says that it is attended by coryza.

It is admitted by all, that surgeons who attend upon patients with granular lids but very rarely contract the disease. This fact and some others of the same kind seem to wholly exclude the supposition that the material of contagion can travel through the air. The risk of contagion appears to be closely connected with the bedchamber. Those who occupy the same room with an infected person at night, and especially if sleeping in the same bed, are very prone to become attacked. This is explained by many observers on the supposition that the closeness of the atmosphere and defective ventilation are important factors. When we remember, however, how various are the possibilities as regards direct transference by contact—towels, handkerchiefs, pillow-cases, flies,† &c.; we shall, I think, be

* See Knapp's 'Archives,' xv, p. 441, 1886.

† My own belief is that flies are the chief agents in the contagion of this disease. Such a supposition explains well the observed fact that it seems to

reluctant to admit, in the face of much negative evidence, that such is really the case. At one of the schools which I have visited, and when the disease had prevailed extensively, the bedrooms were everything that could be wished. In this instance some of the maidservants who attended to the bedrooms had suffered, whilst those employed exclusively in the kitchen had escaped. I know a case in which a gentleman caught the disease from his wife although they occupied a large and airy bedroom. I am indisposed to believe that, excepting by increasing the risk of contagion, crowding, defective ventilation, or any other hygienic condition has any influence in the production of the disease.

There is not, so far as I know, any evidence whatever in support of the idea, that the disease can prevail as an epidemic in a town or community amongst individuals who are not brought into personal communication with each other.

Good reasons may, I think, be alleged for believing that the cause of the ophthalmia which is followed by granular lids is a special or, if we may venture on the word, a "specific" one. This state of lids does not follow simple congestion of the eye from injury or exposure, however long continued. Granulations are no part of the changes which lead to xerophthalmia, or what is called pemphigus of the conjunctiva. Nor do we fear them after syphilitic keratitis, or any of the forms of ulcerative or pustular ophthalmia which occur to strumous children. During some of these latter the eyes may be intensely congested for months together, but as a rule no trachoma results. When the disease passes off, the conjunctiva often—I think I may say usually—returns to a condition of perfect health. Similar statements are true of acute purulent or gonorrhoeal ophthalmia.

spread to the occupants of adjacent beds. It explains the occurrence of epidemics during hot weather, and especially the unusual development of the malady during the summer of 1893, which was very dry, and during which flies were unusually plentiful.

Sporadic cases of granular lids are probably very rare. We almost invariably meet with them as parts of an epidemic, or in those who have suffered at school. I speak of this matter from no very large experience, and there are those present much more competent to give opinions. To specialists, even rare things sometimes come to seem common, yet I much doubt whether any specialist present will be able to tell us that he sees many cases of granular lids in private practice. I have myself treated but few. Those which I have seen have been almost invariably in persons either of Hebrew or Celtic descent. An exception has recently offered itself in the case of a husband and wife, both of whom suffered for long, and in whom I could trace no cause.

The experience of those who have observed this form of ophthalmia as it has occurred amongst soldiers is strongly in favour of its specificity. A misconception prevails, I believe, to a certain extent, as to the nature of the "military ophthalmia." It is supposed from the term purulent, which has been sometimes applied to it, that it is originally connected with contagion from gonorrhoeal discharges. We cannot too soon rid our minds of any suspicion in this direction. The ophthalmia which prevailed in the army in Egypt, and which afterwards proved such a scourge in the armies of Prussia, Holland, and some other countries, was no other than a severe type of that, which in our workhouse schools leads to granular lids. Its ordinary end was granular lids and pannus. It spread by contagion from person to person. It was by no means always attended in the first instance by an acute stage of purulent discharge. This fact may be illustrated by mentioning that the medical officers of the Dutch army were, during its prevalence, divided into "compressionists" and "contagionists." The term "compressionist" meant, one who thought, that the eye disease was caused by the compression of the soldier's collar and helmet strap, a cause to which no one would have dreamed of attributing an acute purulent affection. It may also be borne

in mind, as already stated, that neither the purulent ophthalmia of infants nor that of adults results in granulations.

We may then, I think, without reserve accept the terms "Egyptian ophthalmia" and "military ophthalmia" as synonymous with school ophthalmia, and as applicable to a disease which is caused by a specific virus. It is quite probable that different epidemics may differ considerably in virulence, but allowance being made for this, the experience gained in the one is applicable to the others. Amongst the many valuable papers which have been written on military ophthalmia, we have a long and very complete one from the pen of MM. Testilin and Warlomont, the translators of Mackenzie's work on 'Diseases of the Eye.' This paper gives us a detailed picture of the disease in all its stages, and discusses with great minuteness the various measures of prophylaxis and treatment. Especially is it valuable for the proof which it contains that the disease is contagious, not alone in its early stages, but in its most confirmed. At one time the Belgian Government had adopted the perilous practice of remitting to their homes soldiers who had become disabled by granular lids. Over and over again these unfortunates spread the disease in their families, and became the centre of village epidemics. The danger of contagious spreading of this kind is of course far less amongst the classes which supply pupils to our private schools than it is amongst the poor. There is amongst our own middle class no crowding in bedrooms, and but little risk of the use of towels, handkerchiefs, &c., in common. Contagion from granular lids in a chronic stage will probably occur only by accident, whereas in the cottages of the poor it is almost a certainty. I do not find in the records of workhouse epidemics in our own country, that any compunction seems to have been felt as to permitting chronic cases to return home, and no very startling proofs of the danger of the practice appear to have been recorded. It is, however, high time that attention be given to a most reprehensible line of conduct, since it is very possible that

to it we owe the increased prevalence of the disease which appears to threaten us.

The facts as regards military ophthalmia would lead us to believe that the disease acknowledges no predisposing influences, but may occur to anyone who is exposed to its contagion. The conditions often quoted as likely to be predisposing, such as small, close rooms, crowding, and the like, are obviously precisely those likely to favour direct contagion.

That "military ophthalmia" was, at any rate in some of its outbreaks, a far more severe disease than any which we have now amongst us will be admitted at once by all who are familiar with its literature. In many cases its onset was severe, and such remedies as bleeding and leeches were sometimes resorted to. It led, however, to the same results—granular lids and pannus, and not, as does purulent ophthalmia, to sloughing of the cornea. Its subjects continued to be sources of contagion for many months, or even years. In contrast with present epidemics, it may be plausibly held that its severity was only a matter of degree. It is possible that a peculiarly virulent form of poison had been imported from Egypt, and facts would favour the belief, and both on the Continent and amongst ourselves its virulence has been on the decline ever since. I am told that in Belgium the severe cases described by writers of half a century ago are no longer met with.

One of the most difficult questions connected with the subject is to determine when and under what conditions granular lid ophthalmia may be safely neglected. By neglect is meant allowing its subject to mix with others, and abandoning systematic treatment. Now, although it may be admitted that many cases in slight stages probably recover without much and sometimes without any treatment, and that in a still more numerous class, although the patient himself remains uncured, yet there is no spreading of the disease to others, we must not shut our eyes to the reality of the risk encountered. Granular lids once established are the cause of much misery to the patient,

They entail painful and often repeated measures of treatment. They incapacitate to a very considerable extent for most of the pleasures and duties of life. Those who suffer from them are often for years together unable to read. They are also compelled to live in a state of semi-isolation. In the English race the disease but rarely becomes a source of danger to sight, but amongst others more than a very few eyes are lost. The expressions used by those who have had to deal with epidemics amongst soldiers, may convey some idea of the estimate of their importance which had been forced upon them. The authorities of workhouse schools have, I believe, not as yet taken adequate cognizance of the risk of contagion. Not only have children suffering from ophthalmia been admitted into the schools, but those only half cured have been allowed to leave without remonstrance. It may be difficult to prevent this, but surely some attempt should be made. For all classes of the community, but especially for workhouse children, there ought to be provided hospital schools into which chronic cases can be received, and where they can remain until quite well. At these institutions education should be mainly *vivá voce*, and the reading of books should be minimised. One of the most curious bits of evidence which I have obtained in connection with this topic has been furnished to me by the very intelligent surgeon to one of our largest public schools. This gentleman boasts that the school which he advises is now perhaps the best as regards sanitary arrangements in England, but he adds, "I still have every year forty or fifty cases of workhouse ophthalmia." He attributes them to overcrowding in dormitories—an explanation which, excepting that it favours contagion, I cannot accept. His treatment is the strictest isolation—"as I would with scarlet fever or smallpox"—and the use of nitrate of silver. By these means he holds that he has always succeeded in curing the cases and preventing an epidemic. His remarks imply that under these prompt and energetic measures he has not had any cases

lapse into a chronic state. I find it, however, impossible not to suspect that the permanent prevalence of the disease in his school is to be explained as the result of contagion from boys incompletely cured and permitted to return prematurely to their classmates.

One of the subjects respecting which I hope to gain knowledge is as to the experience of other middle-class schools in respect to what we have hitherto called workhouse ophthalmia. I have told all I know, and I hope others will do the same. As I have already said, there is no need to risk injury to individual institutions by mentioning names. Vague rumours have reached me concerning several schools other than those to which I have referred. Thus I have been told that in one all the masters have suffered. In several instances a single child has been recognised as the subject of granulations who was a pupil at a boarding-school where no special notice was taken of it. I cannot help suspecting that a skilled inspector of the middle-class schools might reveal a far wider prevalence of the malady than we at present recognise. This, however, is conjecture, for I have no proof beyond the facts which I have mentioned. In some instances I suspect that school authorities have concealed the existence of the disease, and in others have simply neglected it.

I shall advert but very briefly to the history of ophthalmia in workhouse schools, since the chief object of my paper is to ask attention to its occurrence in those of another class. It may be of interest to note that this history does not go very far back; just as military ophthalmia was not very long ago a novelty in armies, so workhouse ophthalmia was very possibly a novelty in the middle of the present century. We cannot, of course, feel sure of this. The first report on it was, I believe, one by Mr. Bowman in 1858, and it was followed by others by the same authority, and by Mr. Haynes Walton in 1861. The latter gentleman evidently thought very lightly of it, much as I believe some advisers to our middle-class schools do now. He attributed it, not to contagion, but to cold

and damp, and did not recognise the danger of granular lids. My own report was in 1866, and it concerned a prevalence of the disease which in that school dated back only six years. Although, as others had done, I thought the ophthalmia one to be easily cured, and did not rate its dangers so highly as I should do now, yet I am proud of the fact that I then asserted, that contagion and contagion only was its cause, and recommended then, as I do now, strict isolation as the only means of preventing its spread.

None of the measures which resulted from the reports which I have referred to, or from the subsequent labours of Mr. Nettleship and others in respect to the matter, have proved sufficient. Our workhouse schools still suffer from ophthalmia to a most lamentable extent, and the state of things still loudly calls for additional effort.

I do not think that there can be any doubt that the disease underwent a very great increase during the period that isolation was wholly neglected in the middle of the century, and that it still is terribly prevalent just in proportion to the neglect of this measure. We have in these facts a very definite lesson as to what is now desirable. Unless precautions are promptly taken, workhouse ophthalmia will very shortly become an appendage of our middle-class schools just as it has for long been of those for paupers. From schools it will spread to private families, and, unless something in our race peculiarities protects us, bad cases of trachoma may become as common with us as they have long been in France and in Ireland. No one who has attended the practice of a Paris ophthalmic clinic, and seen what Mr. Critchett described as "the noble army of martyrs standing in rows and waiting in turn to have the eyelids brushed," can fail to appreciate the urgent necessity for doing all that is possible to prevent the development of such a state of things. We must not presume too much upon racial advantages, for the facts to which I have adverted disclose, I think, very serious danger.

In speaking of the preventive and remedial measures which should be adopted when cases of ophthalmia occur in schools, we deal separately with the two stages. There will probably be no difference of opinion as to the desirability of prompt isolation of all cases of inflamed eyes occurring in schools. We cannot be sure which may prove contagious, and if a pupil have red or irritable eyes, isolation and suitable local treatment should be at once resorted to. All large schools should be provided with apartments ready to hand for this purpose. In the majority of cases a cure will probably be quickly effected, but a careful examination of the lids should be made before the pupil is allowed to associate with others, and should be repeated a few weeks later whether there be symptoms or not.

Questions of much greater difficulty present themselves if any condition approaching to granular lids is discovered. The disease may be very slight, or even doubtful. If, however, there be any suspicion of such changes, not only ought isolation from the healthy to be maintained, but additional attention should be given both to local and constitutional treatment. It is probable that the liability to lapse into a chronic state—to pass from the papillary to the sago-grain stage—depends very much upon the vigour of the patients. In such a state tonics and fresh air become of great importance. The place of residence should be dry and bracing, and if the location of the school cannot claim these qualities the affected pupils should be sent elsewhere.

If an epidemic prevail in a school, ought the establishment to be broken up and the pupils sent home? The answer to this question ought, I think, to be an emphatic negative. The disease is not connected, as regards its cause, with the location of the school or with the buildings. It is due to contagion only. If parents insist on having their children home, it must be on their own responsibility, and with a full knowledge of the risk of introducing the disease into their own families. Apart from the danger

of spreading the malady broadcast in the community, we must remember, that by retaining the pupils at the school and under the charge of one surgeon we make more sure of their having skilful treatment efficiently carried out.

Those in charge of schools in which cases of granular lids have been developed will naturally inquire as to the probable length of the isolation which will be required. In cases in which the conditions have not gone beyond the stage of papillary granulations we may reply that a month or two will probably be sufficient. If, however, true trachoma bodies—the sago grains or frog spawn—are present, then the period will be at least one of many months, and it may be years. For such patients special educational arrangements are demanded, for no prudent surgeon would admit them again into a school unless after very prolonged treatment. If the condition had become chronic, and the sago grains had resisted treatment, the child ought perhaps to be regarded as permanently unfit for any school where healthy pupils were received. The treatment of such a case would very probably extend over the whole period usually allotted to school life, for the risk of relapse after apparent cure is very great. Such children should be sent to invalid schools at the sea-side, or in the most bracing situations which can be obtained. It is a question not only of preventing them from being sufferers from irritable eyes for life, but of their being permanent sources of danger to others. It is worth while to adopt any measures, however expensive and troublesome, in order to ensure their cure.

In conclusion, gentlemen, I can only hope that you will not consider me an alarmist. This Society has already done excellent social work by its appeals to those in authority and to the public generally in reference to the prevention of the ophthalmia of infants. As a cause of blindness on the Continent the ophthalmia of granular lids stands second only to purulent ophthalmia itself. We are, I believe, threatened with a large increase of the malady in England,

and I cannot but think that a great duty and a large sphere of usefulness lie before us in respect to it. In the first place we must be well informed ourselves, and in the next we must inform the public. In the term ourselves I comprise not only the members of this Society, or those skilled in ophthalmology, but the whole profession. If I wished to enforce the necessity for general education in ophthalmology, I know of no department from which I could more easily draw good illustrations than that of the prevention of granular lids.

The following were the propositions submitted by Mr. Hutchinson at an adjourned meeting for the discussion of the preceding paper.

Propositions, Definitions of Terms, &c., in Reference to School Ophthalmia.

1. That the contagious ophthalmia which has recently prevailed to a greater or less extent in certain high-class schools is precisely the same malady as that which in pauper schools has long been recognised as the origin of trachoma.

2. That the reason why but few cases of trachoma have resulted from these outbreaks is that efficient treatment has been early adopted.

3. The epidemic to which I referred in my paper as having recently occurred at —— School was, excepting that the cases were of somewhat minor severity, precisely the same in character as that on which I reported in 1866 as then present in the Brentwood pauper schools (see 'Ophthalmic Journal,' vol. v, p. 334). Not a few of the cases at —— School had persisted for several months, and after a certain amount of treatment; many relapses had occurred, and in some instances after a medical certificate that the cure was complete.

4. Neglect isolation, and school ophthalmia will spread through the establishment.

5. Neglect the local treatment of school ophthalmia,

and a certain number of trachoma cases or "workhouse ophthalmia" will be the result.

6. A certain number of protracted trachoma cases have already resulted from ophthalmia epidemics in middle-class schools, and a far larger proportion of cases would have passed into this stage had it not been for careful treatment.

7. It is a feat of clinical imagination merely to conceive that there are two forms of epidemic follicular ophthalmia, one of which has no tendency when neglected to end in trachoma (see Nettleship, 'On Diseases of the Eye,' p. 83).

8. "Follicular conjunctivitis" and "follicular ophthalmia" are names for an early stage of granular lids. This stage is characterised by general enlargement of the normal follicles, and often of the papillæ also.

9. It is admittedly extremely difficult to say in slight cases whether enlarged follicles are a personal peculiarity or an evidence of disease.

10. "School ophthalmia" is a name for a contagious and often very brief form of conjunctivitis, which, if neglected, may in certain cases produce granular lids.

11. "Workhouse ophthalmia" is a name which has been used for cases of granular lids (trachoma). It is always initiated by an attack of "school ophthalmia."

12. The disease known conventionally as "catarrhal ophthalmia," which often runs through families, and is attended by patchy congestion of the ocular conjunctiva, may possibly be the same disease as "school ophthalmia."

13. "School ophthalmia" is, however, rarely attended by much congestion of the ocular conjunctiva.

14. The initial attack is usually easily and completely cured by local treatment.

15. The second stage, "follicular ophthalmia," is usually curable by efficient treatment perseveringly used for a month or two.

16. The third stage, that of "trachoma" (when the conjunctiva is thickened and the follicles extensively im-

plicated), is curable with great difficulty, very liable to relapse, and often requires a treatment extending over years.

17. In all stages those who suffer from granular lids are liable to relapse, and to become sources of danger from contagion.

18. The danger of contagion is probably in proportion to the amount of puro-mucous secretion. During conditions of dry eye, with little or no secretion, such cases are probably but little dangerous.

19. School ophthalmia by no means always leads to granular lids; on the contrary, a very large majority of its subjects recover completely, and such recovery is almost universal under efficient treatment.

20. There is, however, always a risk that school ophthalmia may, if neglected and in certain constitutions, run on into granular lids.

21. Conversely, cases of granular lids are in all stages, if temporarily attended by discharge, liable to become the sources of epidemics of school ophthalmia.

22. It is probable that all epidemics of contagious ophthalmia in schools are the same in kind, although they may, in accordance with the general laws of epidemics, differ widely in severity of type.

23. The tendency to the development of granular lids after an attack of school ophthalmia probably depends much upon the race-descent, structural endowments, and state of vigour of the patient.

24. All cases of trachoma have been preceded in the first instance by an attack of conjunctivitis due to specific contagion.

25. The initial attack of conjunctivitis is in its nature transitory; it is often slight, and lasts only a week or ten days.

26. After the initial attack the conjunctiva of the globe may resume a normal condition whilst changes are going on insidiously in the follicles and papillæ.

Memoranda for the Guidance of School Authorities.

A certificate of freedom from ophthalmia should always be required on the return of a pupil to school (as is already the case in reference to tinea and some other maladies).

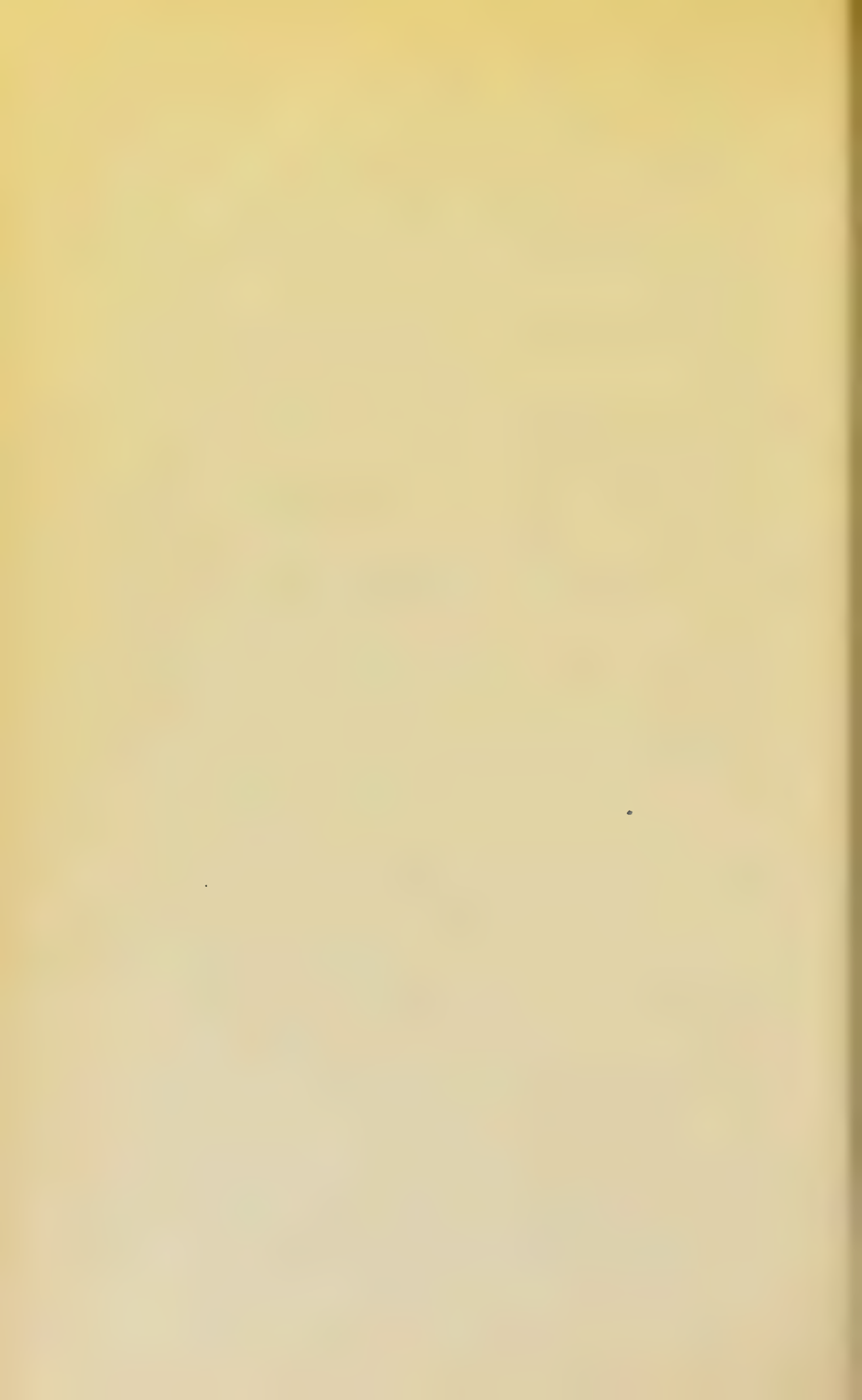
In the case of anyone in a school suffering from "sore eyes," isolation should be immediately adopted and advice obtained.

If in any case relapses occur and granular lids are developed, the isolation and treatment ought to be prolonged, and the pupil ought not to be allowed to return to school until the cure has been established (without relapse) for several months.

No case of granular lids, still requiring treatment, should ever be admitted into a school where there are healthy children.

In case of an outbreak in a school the treatment should be conducted on the spot, and the pupils ought not to be sent home.

The treatment need not interfere materially with school routine.





Reprinted from the 'Transactions of the Pathological Society of London,' 1888.

*On some examples of Arsenic-Keratosis of the skin and of
Arsenic-Cancer.*

By JONATHAN HUTCHINSON, F.R.S.

(With Plates XXI, XXII, and XXIII.)

IN the following statement I have two separate propositions to maintain, one of which is, however, of great importance to the other. The first is that by the prolonged internal use of arsenic the nutrition of the skin may be seriously affected, and that, amongst other changes, warty or corn-like indurations may be produced. The second goes much further and asserts that if the drug be continued these "arsenic corns" may assume a tendency to grow downwards and pass into epithelial cancer.

It will be convenient to begin with to produce evidence as to the first of these assertions. A few years ago a gentleman of nearly sixty was sent to me by Dr. H. Weber on account of an eruption which had apparently followed the use of arsenic. He was the subject of malignant disease of the right tonsil and glands of neck, and in order to restrain its growth arsenic had been prescribed in large doses. For several months he had been taking regularly Fowler's solution in twelve-minim doses three times a day. Under its employment he had emaciated, had suffered much from stomach neuralgia, and his skin, which previously had been perfectly healthy, had become everywhere of a brown, earthy tint, and covered in parts by scaly patches like psoriasis. The scaly condition was most obvious on his elbows and backs of knuckles. The drawings now exhibited show the state of these parts very correctly. The palms of his hands had become dry, slightly horny, and covered by little pits

where the epidermis had broken up. In some spots the epidermal layers were thickened into small hard corns (see Plates XXI and XXII).

The arsenic was at once discontinued, with the result that the stomach pains ceased and that the state of the skin was definitely ameliorated. The drug was afterwards resumed and the skin got worse. This patient, who was, as already stated, the subject of cancer before the arsenic was given, never developed any fresh cancer in connection with his arsenic corns. He died of his original disease a few months after I first saw him. The point which I wish to establish in connection with his case is that the state of skin was without question due to arsenic, and that it was exactly like that which, in the next case proved to be introductory to cancer.

The subject of the next case was a well-known member of the medical profession in Boston, U.S. His case has already been ably recorded by Dr. James C. White, of Harvard University, under the title of "Psoriasis—Verruca—Epithelioma: a Sequence." I have nothing of importance to add to Dr. White's narrative, excepting the suggestion that the verrucæ, or rather corns, which preceded the epithelioma, were themselves caused by the long-continued use of arsenic for the cure of the psoriasis. Briefly, the facts are these: Dr. W—, then aged 44, came over to England for advice in December, 1883, with his hands in the condition shown in plate (Plate XXIII). With the exception of the addition of new growths (cancer) they were the counterparts of the hands of Mr. D—, the subject of the preceding case. There was the same general dryness, the same tendency to form little pits in the hardened epidermis and to produce little dense corns.¹ Dr. W— had been the subject of psoriasis from boyhood, and had taken large quantities of arsenic. The psoriasis, which was of the ordinary form, did not originally affect his palms (a very rare event), and the state observed in them had only come on after long use of the drug. On this important point Dr. W—'s testimony was clear. The cancerous processes were yet more recent and had been present in the right palm about three years before I saw him. As Dr. White witnessed the whole

¹ Dr. White's description of these warts or corns is as follows: "Sharply defined, prominent, firm, and horny outgrowths, some of them resembling the unpigmented formations of keratosis senilis in the same localities, whilst others were more like callosities or some form of warts."

process of development, it is better that it should be described in his own words:

“Three years ago one of these outgrowths, situated upon the right palm near the wrist in one of the great longitudinal dividing furrows of the skin, became excoriated, either by abrasion or fissuring, which refused to heal by the simple measures employed, and terminated in a small ulcer. It remained in this condition many months, sometimes nearly filling up and protecting itself with a thin epithelial cover, sometimes breaking down again and remaining in an open indolent condition, but the destructive process gradually extended more deeply into the cutaneous tissues, and widened its borders. After a time the peripheral portions of the integument became indurated and thickened, forming a dense and elevated circumvallation about the central ulceration. During this long period the efforts to restore the part to a healthy condition were persistent and various; at first simply soothing applications combined with restraint to the movements of the part, afterwards more stimulating applications, then cauterizing agents, as chromic and concentrated nitric acids, and later the curette. Finally, as the ulcer enlarged and deepened, so that the integument involved in the destructive process and surrounding induration was one half inch in area. The whole growth, in August, 1883, was deeply scraped. At the same time another lesion, identical in history and character, but of much smaller size, which had more recently and gradually established itself upon one of the warty hypertrophies situated upon the palmar fold of skin between the fore and middle fingers of the left hand, was also thoroughly curetted.”

Dr. W—, whilst in London, saw Sir James Paget and myself, and microscopic sections, which he brought with him, were carefully examined by us, and were also made the subject of a critical report by Dr. Creighton. The conclusion arrived at was that they were certainly epitheliomatous. He next went on to the Continent and obtained the opinions, amongst others, of Professors Billroth and Kaposi at Vienna. He afterwards returned to New York, where, in accordance with the recommendations given him, his right forearm was amputated. This was done by Dr. Hodges, and at the same time the affected fingers of the left hand were removed.

The sequel proved the malignancy of the disease, for he died

about sixteen months after the amputation with growths in the axillary lymphatics and internal organs.

I quote from Dr. White's paper the description by Dr. Fitz of the examination of the amputated parts.

"The palm of the right hand presented an elevated, rounded, ulcerating mass with dense everted edges and irregularly scalloped outline. The surface for the most part was smooth, reddish-gray, and translucent, but showed an irregular, deep, sinuous depression at the upper and outer fourth. It measured two and a quarter by three and a third inches, and the mass projected two thirds of an inch above the cutaneous surface.

"On section the superficial ulcer corresponded with a circumscribed new formation, one and a quarter inches in thickness, extending through the skin and subcutaneous fat tissues to the deep fascia, being intimately united to the tendinous sheaths, which were not perforated. The growth was continued into the substance of the unciform bone and into the abductor minimi digiti. The cut section was in general relatively homogeneous, grey, and translucent. Minute ecchymoses were present near its free edge, and occasional opaque lines extended upwards and outwards from the base. Pressure caused the escape of small, soft, opaque white plugs.

"The microscopic examination showed that the structure was composed of variously shaped, anastomosing bands of cells, resembling the deeper layers of the epidermis, and separated by a framework of fibrous tissue. The bands contained numerous onion-shaped bodies of laminated epidermoid cells, and extended irregularly in all directions.

"At the palmar base of the fore and middle finger of the left hand was a small superficial ulcer upon the surface of a dense rounded and flattened nodule, one by one and a half inches in length and breadth, and three fourths of an inch in depth. The cut section of the nodule showed a grey, slightly translucent new formation, traversed by occasional fibres. The growth extended into the subcutaneous fat tissue, and the tendons beneath were freely movable. The structure was like that of the growth on the right hand, though the fibrous portion was relatively more abundant, and in both the characteristic appearances of flat cell epidermoid cancer were presented."

The appended letters which give full details as to Dr. W--'s last illness and the autopsy were kindly sent to me by Professor

Bigelow.¹ In addition to the information which they give Prof. Bigelow states that for some days before death there was acute pain in the cervical vertebræ. This region was not examined at the

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Boston; April 26th, 1886.

My dear Professor Bigelow,—The principal facts in the case of Dr. J. H. W.—are these, viz.:—That about a month after seeing Mr. Hutchinson, in London, his right arm was amputated in the middle of the forearm. At the same time the fore and middle fingers of the left hand, with parts of their respective metacarpal bones, were also removed. These wounds healed rapidly. The cicatrices and tissues in the vicinity of both operations remained perfectly healthy until the patient's death. Eventually a lymphatic gland, enlarged to the size of a nutmeg, appeared above the left elbow. This was freely excised. Under the microscope this new growth was shown to be an epithelioma. The operation was quickly recovered from. A few months after its performance a severe and sudden "pleuritic" attack, unaccompanied by decided physical signs, confined Dr. W. to his bed for a few days. Although of brief duration, this sickness was followed by a dry and hacking cough, which never disappeared. In the latter part of 1885 a neuralgia, about the neck and right shoulder, wore upon the patient's strength and health, which had previously been excellent, and to the pain and exhaustion incident to its unremitting severity, he gradually succumbed at the end of the year.

At the time of Dr. W.'s death no reappearance of the local disease was visible externally, nor was there tangible evidence of its existence anywhere, except in an enlarged gland in the left axilla. I believe you have a report of the post-mortem examination, describing the internal lesions which were discovered.

I am, very sincerely yours,

Professor Henry J. Bigelow.

R. M. HODGES.

Notes of the autopsy by Dr. Fitz.—The post-mortem examination was made 6th January, 1886. Body emaciated. In the left axilla was a dense flattened nodule, near, but not adherent, to the skin, as large as a dried fig. Nothing abnormal on examination of the pericardium and heart. The anterior surface of the left lung was adherent to the thoracic wall by recent adhesions. In the posterior and lower portion of the left pleural cavity were about three pints of opaque, reddish-grey fluid. The pleura was thickened, opaque, besmeared with a soft yellow false membrane. In the posterior and upper part of the left lower lobe was a firm rounded mass as large as a small orange. On section a central cavity was found, representing the greater part of the volume of the mass, and filled with an offensive, opaque grey fluid. The wall of the cavity was about a half inch in thickness, dense, yellowish grey in colour; its free surface was irregularly nodulated and granular. A dense, yellowish-grey nodule, without central softening, was found in the lower part of the left lung. At the root of this lung, but within the lower lobe, the main bronchus was surrounded by a dense neoplasm, resembling those already described, and extending outwards a half inch or more. The included bronchus was irregularly narrowed, and its mucous membrane deeply injected and infiltrated with blood. Elsewhere in the

post-mortem. It is very probable that a growth was forming there.

Dr. W—'s psoriasis began at about twenty, and he soon afterwards commenced the use of arsenic. Corns began to form on his hand when he was thirty-four, they ulcerated when he was forty-two, and the first scraping was done at forty-four. He came to England in the same year. The amputation was eight months after the scraping, and he died eighteen months after the former.

That the palmar keratoses were in this case the seats of the cancerous development no one will, I think, hold doubtful who reads Dr. White's description of the early stage of the latter, and who examines the portraits of the two hands. It will be seen in the portraits that the conditions in each are precisely similar, and thus we had a double illustration of the facts. That the keratoses were in their turn due to arsenic no one will, I think, doubt who has given attention to the preceding case.

I have next to mention three other cases from my own experience in which cancerous processes followed the use of arsenic. Two of these are of the more value because they occurred long before I had

lungs nothing abnormal was found with the exception of œdema. The bronchial glands were not abnormal in appearance.

In one of the ribs of the left thorax, near the spine, was a central nodule of new formation, as large as a walnut, apparently myelogenous, which had caused an expansion of the rib, with an absorption of the lime salts in the cortex.

Nothing abnormal found on examination of the spleen. The left kidney, not particularly enlarged, contained numerous round nodules, dense, opaque, grey, varying in size from that of a pea to that of a marble. The right kidney increased in size, at least one third, presented similar changes, the nodules being more numerous and larger. A nodule of new formation, as large as a marble, was found in the perinephritic fat tissue in the vicinity of the right renal artery, and some of the lumbar lymphatic glands were moderately enlarged and indurated. The right suprarenal capsule was transformed into a dense yellowish-grey mass, as large as a prune. The left capsule apparently normal. The upper surface of the right lobe of the liver showed a grey nodule, as large as a dried pea. There was a moderate degree of fatty infiltration of the liver.

The microscopic examination of the nodules in the lung and kidney showed an alveolar structure. The alveoli were often small, the stroma abundant and dense. The contained cells were epithelioid and small.

Diagnosis.—Cancer of the left axilla, of the left lung, of a left rib. Cancer of the kidneys, right suprarenal capsule and perinephritic fat tissue, lumbar lymphatic glands and liver. Acute pleurisy; œdema of the lungs.

(Signed) R. H. FITZ, M.D.

formed any opinion as to the action of arsenic in this direction. I have taken them out of old note-books, where they had been recorded without any special theory in view.

In the first of these the palms were affected by corns, but the cancer occurred on the scrotum. A clerk named Edward B—, aged 34, was at the Hospital for Skin Diseases in August, 1871. He had been long under Mr. Hunt's treatment by arsenic for psoriasis. There was cancer in his family. The palms of his hands were speckled with numerous whitish spots, which were slightly raised, "just like so many soft corns," "no warts." The corns were specially marked in the palms and where the fingers rubbed together. The soles of the feet were in a similar condition. He had noticed this condition for eighteen months, and it began after he had been some years under Mr. Hunt's treatment.

At the Hospital for Skin Diseases we continued arsenic, not then knowing any better.

In February, 1872, he had a hard-edged sore on the scrotum, which had been preceded by a wart or corn. In June this sore was recognised to be epithelial cancer, and early in July he was taken into the London Hospital, and had it excised. At this date the corns in the palms had to some extent "peeled off."

In April, 1873, there was no return and no enlarged glands.

I am sorry that I have not been able to obtain more recent information as to this patient.

The following notes concern a patient whom I never saw, but about whom I was consulted by letter by Dr. Clifford Allbutt, of Leeds. The lady died not long after the notes were written.

Miss H—, when aged 5, came under Dr. Clifford Allbutt's care for pemphigus. It was a very bad case. She took arsenic, and it stopped the eruption. The latter, however, relapsed, and she got into the way of taking arsenic herself. The inner aspects of her hands and fingers became "very rough." She married before twenty, and had a child born.

When twenty-five she came again to Dr. Allbutt with a large, open ulcer behind the crest of the ilium on left side. She said that there had been a roughness on the spot, and that she believed the sore had been caused by her clothes hitching. There was a secondary glandular swelling in the groin. Subsequently a lump formed in the skin of the inner aspect of the thigh, "hard, yellowish reddish and purple mottled, but not pigmented." At this stage the gland

mass had ulcerated, and it, as well as the original, were described as like a tomato in colour and shape, having lumpy, everted edges.

No opportunity for microscopic examination ever occurred, but that the patient died of malignant disease, sarcomatous or epithelial, there could be no doubt. There was equally little that her skin had been made rough by the long-continued use of arsenic.

Another example of cancer following keratosis came under my notice in the person of gentleman, aged 55, from Edinburgh. In him the growth had occurred on the sole of the foot, and the appearances were closely similar to those exhibited in the portrait of the Boston case. (A portrait of sole of foot was shown at the meeting.) Anyone looking at the portrait might be inclined to suggest "perforating ulcer," for the ulcer is in exactly the position in which that ulcer usually occurs, and it has thickened edges which are not dissimilar in general appearance. It was, however, most certainly not of that nature, for the ulceration did not pass deeply and there was definite new growth around it.

The history which I received with this patient was that he had for many years been the subject of psoriasis and had taken much arsenic. "For many years he has been troubled with a tendency to formation of corns on hands and feet and general hypertrophy of cuticle. The skin becomes dry and harsh. He has lost to a great extent the power of feeling in the right hand, so that he does not know when he is grasping a pen, and has to write with his left hand." In December, 1886, the following note as to the state of his foot was made: "The skin of the sole of the foot is dry and rough, and over the ball of the great toe there are two ulcers the size of three-penny bits, the skin around which is much thickened." . . . "The patient states that these ulcers are in the place where for several months he had two hard corns which he used to have pared." That the ulcers which Mr. — suffered from were of no ordinary kind may be assumed from the variety of measures of treatment which they had resisted. In addition to numerous local applications the foot had been kept at complete rest in a plaster-of-Paris case for some months and walking had been wholly forbidden, yet no tendency to heal was shown. It was on account of this obduracy that he was finally sent to London.

This gentleman was under the care of my friend, Prof. Chiene,

in Edinburgh, and he returned to him and had the affected part freely excised and treated with chloride of zinc. Microscopic examination of the portions removed did not reveal any elements which were conclusive as to epithelial cancer, but to this negative result I may freely confess that I do not attach much importance as regards diagnosis. After the scraping the sore healed, and it remains, I believe, sound up to the present time (about a year).

The microscopic examination was very carefully made by Dr. George Mackay. I extract the following sentence from his report, it being the one bearing most nearly on the question: "The sections show intrusion of epithelium at the margin but without rupture of boundary zone between epithelium and connective tissue. This may be nothing more than the natural growth of the epithelium and papillæ recently pointed out as tending to occur as life advances (Dr. Woodhead)."

On December 4th, 1887, Mr. Chiene wrote me: "I saw Mr. C—to-day; there is a corny cicatrix at seat of operation, but nothing more; no pain. The corns on that foot are better but the corns on the sole of the opposite foot and on the hands remain as they were—no improvement. He still suffers from psoriasis and has twice taken since the operation a little arsenic."

In Dr. White's paper, to which I have already been so largely indebted, another case is recorded from his own observation of an almost precisely similar nature. A gentleman, aged 52, had suffered from psoriasis all his life and had taken much arsenic. Ten years before coming under Dr. White's care he had noticed that his hands were beginning to assume a thickened warty condition. Some years later an ulcer formed on the front of the wrist which resisted all treatment. Caustics and scraping were freely employed, but in spite of them it passed into a condition which Dr. White describes as exactly like that which is shown in Plate XXIII. Dr. Hodges was also struck with the remarkable similarity between the two cases. A very free excision of the diseased parts was made. Some months later, soon after a second operation for removal of sloughing tendons, the patient sank from acute cellulitis of the arm. This patient had also a small growth near the anus and it was also excised. The most important paragraph in Prof. Fitz's histological report on the parts removed is the following: "The specimen was characterised by the presence of large masses of epithelioid cells of irregular shape and size, separated by narrow

bands of fibrous tissue and extending deeply downwards into the subcutaneous fat tissue."

Dr. White's diligent research has been rewarded by yet another case, which is recorded in 'Gazette des Hôpitaux,' 1878, p. 750. In this Dr. Cartaz reported to the Anatomical Society the case of a healthy man, aged 40, who had for twenty-three years suffered from psoriasis, which had, at length, among other regions, invaded the palms and soles. "In consequence of scratching, the scales from one of these processes, situated upon the palmar surface of the second phalanx of the ring finger of the right hand, were removed, and there remained a little ulceration which gradually extended to the size of 4 centimetres. The callous borders of the deep-seated granulations, bleeding at the slightest touch, established the diagnosis of *cancroïde*. Amputation at the metacarpo-phalangeal joint was performed and there was no return of the disease." The narration of this case is defective in that we have no statement of the intermediate formation of a corn, and no information as to whether or not arsenic had been used. The fact that the man had suffered from psoriasis more than twenty years makes it very probable that he had taken it.

In the preceding statements six cases have been adduced in which cancer occurred in persons who had for very long periods suffered from disease of the skin. In five of these arsenic was known to have been very freely used over very long periods, and in the sixth there is no evidence to the contrary. In five of the cases the skin disease was psoriasis, but in the fifth it was pemphigus. In one case the scrotum was the part on which the cancerous ulcer formed and in one the side of the trunk, whilst in all the others it occurred on the hand or foot (sole once, palm once, and finger once). In all cases a condition of local keratosis preceded the cancer. One of the cases, that of Dr. W—, should almost rank as a double fact, for the keratosis and cancer occurred sympathetically in the two hands. Three of the patients are dead (two of secondary malignant growths), and respecting two others nothing is known as to later history.

The question to be considered is as to whether there be any real connection between the skin disease and the cancer following it, and if so what? The reply which I have suggested is that the bond of connection is to be found in the influence of the drug given for the cure of the skin disease. It seems highly improbable that psoriasis, however protracted, should in any number of cases result in cancer.

We have no clinical experiences to support such a conclusion. The parts affected in a majority of these cases are, besides, precisely those which psoriasis does not attack. It is exceedingly rare for common psoriasis to show itself on the palmar surfaces. Its favourite sites are the extensor surfaces, and when the hand is involved it is usually its back. Yet in most of these cases the palmar surfaces were affected, and keratoses, which are unknown as a part of common psoriasis, occurred late in the disease as a precursor of the cancerous process. In this difficulty the first case which I adduced comes to our aid with evidence which is invaluable. In it changes in the nutrition of the skin, and especially in that of the palms of the hands, were induced by the use of arsenic, and they were precisely like those which in the other cases preceded cancer. Nor does this case by any means stand alone as proof that arsenic can alter the state of the skin. Many others have been recorded in which the integument has become dry, harsh, and muddy during its use, and some in which a great increase of pigment was the result. It may be admitted that the formation of keratoses under its influence is a more novel observation, but it is only going a short step farther. It would appear that the influence of arsenic when used to saturation through long periods is to lower the nutrition of the skin and render it less able to bear ordinary kinds of irritation. Thus we find the palms and soles forming corns, from local pressure presumably, in a most unusual manner. The adjacent surfaces of the fingers where touching each other also form corns, and in one of the cases which I have recorded the part affected had been pressed upon by the dress. Not only does arsenic depress the nutrition of the skin, it may induce local neuritis. That it causes the outbreak of herpes is now, I think, generally admitted, and in one of the cases just mentioned the fact was recorded that the patient had a partial paralysis of one hand. I believe that I have seen more than one case in which arsenic has caused the formation of opacities in the vitreous humour.

In conclusion, a word or two may perhaps be permitted as to the clinical bearing of the facts adduced. Dr. White has very properly insisted that "the lesson to be drawn from their study is that the transformation of patches of psoriasis into verrucous hypertrophy must be regarded as an ominous occurrence, and that the softening or other change of such horny growths demand thorough excision without delay." To this I would venture to add that it is desirable

to take precautions still earlier, and that in all cases in which during the use of arsenic for any skin disease the palms and soles become rough and corneous it is an indication that the use of that drug should be laid aside. If the case when first seen is more advanced and cancer be already threatened, the propriety of amputation in preference to excision must be carefully entertained, for the form of cancer would appear to be one which is promptly infective to the glands and other parts.

December 6th, 1887.

DESCRIPTION OF PLATE XXI.

In illustration of Mr. Hutchinson's paper on Arsenic-Keratosiis and Arsenic-Cancer.

The Plate shows the elbow of a gentleman whose hands are delineated in Plate XXII. It will be seen that the whole skin is of a dirty brown tint and that a large scaly patch has been developed near to the elbow. This patch was quite dry, but it was somewhat less circumscribed and showed more of redness about its edges than common psoriasis usually does. The patient had a perfectly healthy skin previous to the administration of arsenic.



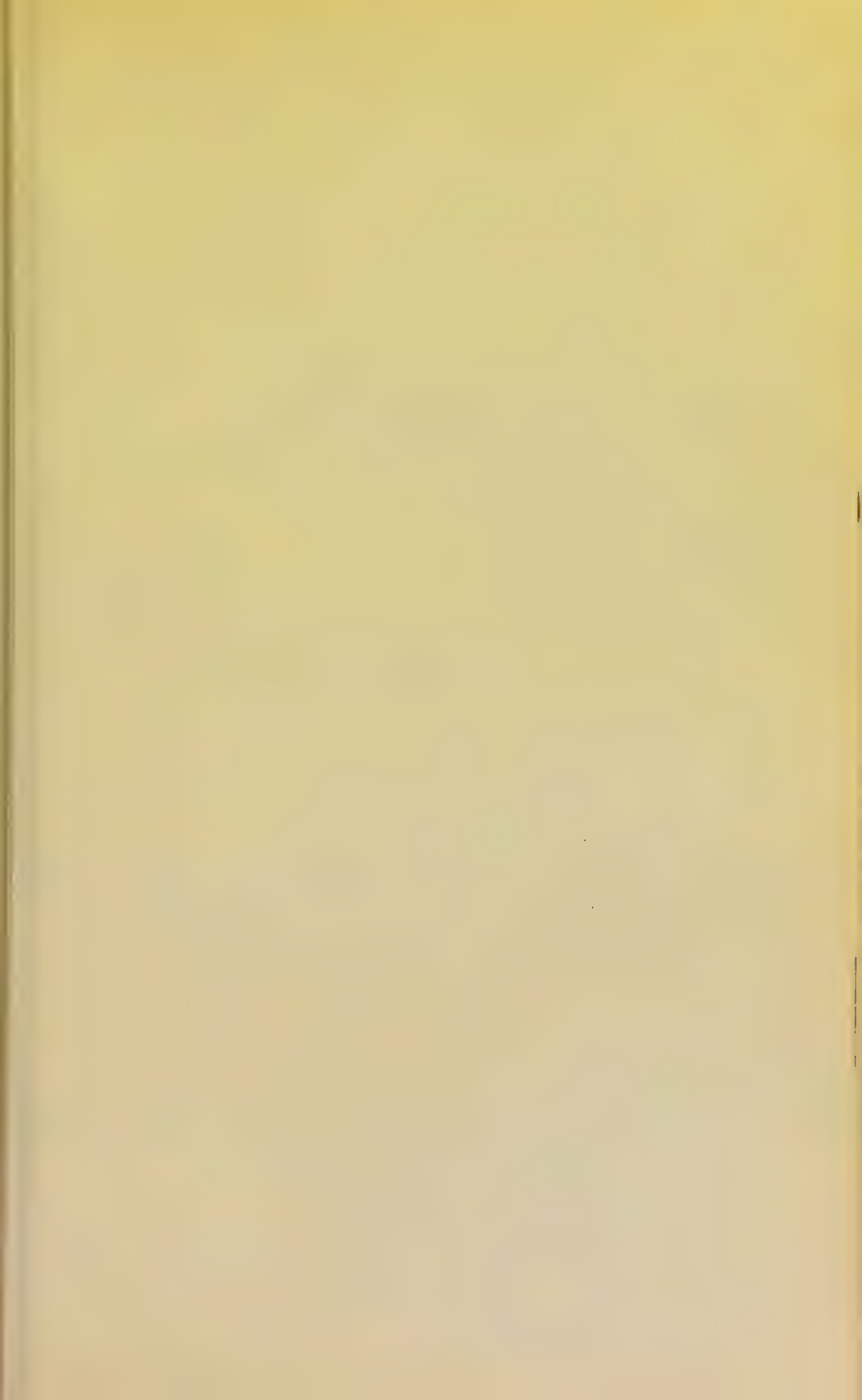


DESCRIPTION OF PLATE XXII.

Illustrating Mr. Hutchinson's paper on Arsenic-Keratosis and Arsenic-Cancer.

The back and front surfaces of the hands of Mr. D—, the patient whose elbow is shown in the preceding plate. It will be seen that across the knuckles of the hand a number of dry, thick, scaly crusts have formed. On the backs of the fingers there are numerous little pits where the epidermis is broken, while the skin generally is discoloured and harsh. The patches across the knuckles are exactly like psoriasis, excepting, perhaps, that there is more thickening beneath the crusts. They do not occur precisely in the parts where psoriasis is most common, that is, on the back of the hand, rather than the knuckles. The palmar surface of the hand shows the epidermis dry and cracked, and there are numerous little pits where it has been broken. The extremities of the fingers are especially affected and somewhat reddened. It has been difficult to represent the little corn-like patches of thickened epidermis which were present.





DESCRIPTION OF PLATE XXIII.

Illustrating Mr. Hutchinson's paper on Arsenic-Keratosis and Arsenic-Cancer.

The palmar surfaces of the two hands of Dr. W—, who became the subject of arsenic-cancer. It will be seen that the palms are exactly like that of the hand shown in Plate XXII, but with the addition of a fungating growth in each. That in the right palm is of considerable size and thickness and is placed just above the wrist. That in the left is much smaller, having been of more recent development, and shows only a reddened excoriation between the index and middle fingers, beneath which there is a certain amount of thickening. In this case common psoriasis had been present in the first instance, and arsenic had been given in large doses over a long time for its cure.



An account of the skeleton of the Norwich dwarf.

By JONATHAN HUTCHINSON, F.R.S.

[With Plates XVIII and XIX.]

THIS skeleton, which is the property of the museum of the Norfolk and Norwich Hospital, was lent to me for exhibition through the kindness of Mr. Cadge and Mr. Cross, the latter of whom is the custodian of the collection. The skeleton is that of a man who was executed for the murder of his child and attempted murder of his wife in August, 1819. The crime was effected by the administration of poison. His age was 35 years. The newspaper account of the case states that "his personal deformity was in singular unison with his moral depravity." His height from the crown of his head to the sole of his foot was four feet two inches. No particulars are forthcoming as to his character previous to the crime for which he was executed, but it is stated that at his trial "his countenance, which was as singular as his diminutive figure, remained unmoved and unaltered." It is added that he was exceedingly attentive to religious instruction, and received it thankfully but unimpressively. "To the last, in spite of his confession, his thoughts, incapable of spiritual reflections, were more employed on his vindication than his salvation." Although but few facts are forthcoming, it is clear that a general impression was formed that he was a man of very inhuman character. This fact becomes of great interest in connection with the singularly brute-like formation of the bones of his extremities. His dwarfed character was almost wholly due to the shortness of his legs, for there was nothing peculiar in the length of the vertebral column. His upper extremities were shortened in like proportion with the lower ones, the tips of his fingers only just touching his great trochanters. It may be briefly stated that almost all the larger bones of his limbs were short and remarkably thick but that none of them were in any way curved as in rickets. They showed strongly-marked projections for the attachment of muscles, and were, indeed, developed on an exceedingly coarse type. His head, although ill-shapen, narrow, and rather high, was not smaller than the average. The following

measurements of his limbs are taken from the account which was published after his execution and before the dissection :

His height from the crown of the head to the foot was four feet two inches.

Measuring from the same point to the top of the breast-	
bone the distance was	13 inches.
From the latter point to the extremity of the trunk . .	19 „
Length of the thigh	8 „
Length of the leg	8 „
Which with the foot made up the whole	50 „
Length of the upper arm	6½ „
Length of the lower arm	7 „
Length of the hand and longest finger	5½ „
Circumference of the wrist	6½ „
Circumference of the knee	14 „
Circumference of the ankle	8 „
Breadth of the shoulders	4 „
Breadth across the hips	12 „
Circumference of the skull	23½ „
Distance from the tip of the chin and crown of the head .	9 „
Circumference of the neck	14½ „
Circumference of the trunk measuring round the lower end	
of the breast-bone	32 „

The following description of the skeleton is in part compiled from detailed measurements which were kindly made for me at the Royal College of Surgeons by Mr. Garson.

General statement as to the measurements of bones, &c.—Bones of trunk fairly normal. Bones of the limbs arrested in growth. Skull long and narrow, smooth, without marked prominences. Sutures open. There is a marked want of expansion of the upper part of the skull, and the frontal bone is very narrow. The spinal column is apparently normal, until the sacrum is reached. This bone is set almost at right angles to the spinal column. The ribs are somewhat heavier and more narrow than usual. The clavicles are arched upwards. The humerus is much shorter and thicker than natural. The radius is short and much bent by exaggeration of the normal curves. The hand and wrist normal in proportion.

The femur is diminished in length; the lower end suddenly expands into the condyles, like the head of a nail. The tibia is short, its shaft being normal. Foot normal, except in size.

The following is the detailed description by Dr. Garson of the

principal bones affected: The humerus is much shorter and thicker than usual. The articular surface of the head is nearly normal; no anatomical neck may be said to exist as the articular surface is sessile on the shaft of the bone. The tuberosities are very strong and project upwards higher than the articular surface. The surgical neck is very thick, with rough and rugged ridges on it for the attachment of muscles. The deltoid insertion is exceedingly prominent, breadth across 35 mm., while the normal is 28 mm. The lower articular end is the same size as that of a normal humerus. The circumference of the surgical neck is 102, while an ordinary bone is 90 mm. The radius short and much bent by exaggeration of the normal curves of the bone. The attachment for the biceps is exceedingly developed, being broad and prominent. Muscular surfaces very strongly marked. The carpal articulation of the radius is larger somewhat than in an adult person.

The pelvic brim markedly reniform, the hilus being formed by the promontory of the sacrum. The breadth of the innominate bones is considerable in proportion to the height. The ilia are shorter than usual as is also the ischii. The crests of the ilia approach more nearly to one another than usual. The cavity of the true pelvis is absent, from the irregular position which the sacrum assumes, the latter arising almost at right angles to the direction of the spinal column. The pubic portion of the pelvis is normal. The sacrum is perfectly straight.

The femur is considerably diminished in length. The acetabular end is somewhat oval, instead of round. The neck is short and compressed from above and below, 24 mm. in width, from before backwards 39 mm. The trochanter major is well developed with rough surfaces for the muscular insertions. The top of the trochanter is higher than the level of the head of the bone. The trochanter minor is of large size, as is also the ridge running from the trochanter major to it. The shaft of the bone is short and has rough surfaces well marked. At its lower end the bone suddenly expands into the condyles, so that the condyles appear to be stuck on to the end of the bone, somewhat like the head of a nail on to the shaft. The internal and anterior surface of the condyle is worn down.

The tibia is short, its shaft is normal, but the articular ends are modified. The anterior surface of the bone at the knee-joint is deficient, just as if the anterior part had been split off transversely and vertically. The whole articular surface is broad in proportion

to the length. The lower articular end is large proportionately. The shaft suddenly expands into it.

Fibulæ, remarkable for the development of all the normal ridges of the bones, which are longer than usual for the limb and bowed. They are projecting backwards; the upper articular end is large.

The following points, in reference to the skeleton above described, appear to deserve particular attention:—First, the entire absence of indications of any stage of softness of the bones, with liability to bend. None of the long bones showed any abnormal curves whatever, nor did they exhibit any of the local developments of bone as splints which are so commonly seen in rickets. They were simply short, thick, and very strong, and with the prominences for the attachment of muscles remarkably large.

Secondly, the bones of the lower extremities, especially the femurs, showed remarkable expansion at their articular ends, much resembling the lips which are developed in connection with chronic rheumatism. It is possible that this may have implied a tendency to arthritic disease in very early life, but against such a supposition we may note that it is entirely absent in the upper extremities.

Thirdly, the alterations in form are not restricted to the long bones. The pelvis was remarkably small and its outlet narrow. The ossa innominata were not like the femora, thick and coarse, but smaller and lighter than naturel. In the upper extremity the lightness and smallness of the scapula and clavicle were very noteworthy.

Fourthly, we must note the peculiarity of the clavicles, which presented a feature which I have never seen in any other skeleton. Instead of the thick knob-like end which the sternal end of a normal clavicle presents, terminating in an almost flat surface, these bones had the lower two thirds of their inner extremities wanting, so that they became almost pointed, and presented below an inclined surface, as if designed to slide backwards and forwards on the upper part of the sternum. Unfortunately no particulars as to the dissection of the sterno-clavicular joints are forthcoming. I do not at present see any special interpretation which can be given to this anomaly.

In speculating as to the causes of the peculiarities exhibited by this skeleton, I am inclined to doubt altogether that they have anything to do with rickets, properly so called. Not one of the features

peculiar to rickets is present, nor does that disease, so far as I am acquainted with it, ever lead to simple arrest in the growth of bones without any accompanying tendency to bend. To the disease which has been described as "intra-uterine rickets" it is, however, quite possible that the case may have some affinity. The question remains, however, whether the name referred to is correctly applied. Many of our museums contain specimens of fœtuses either dead at birth or dying soon afterwards, which show remarkable defects in the growth of the limbs. I am not sure, however, that any of these exhibit the peculiarity of the Norwich skeleton, in that the spinal column, head, and ribs are not affected. I do not know of any adult skeleton in an English museum which shows conditions at all approaching to those which I have described.

Specimens 104 and 116 in the museum of the Charing Cross Hospital are of interest in connection with it. They are the two humeri and one femur of an old woman, of whom no history has been preserved beyond the fact that she was a dwarf. They resemble the bones of the Norwich skeleton in being thick in proportion to their length, and in having the ridges for muscular attachment unusually well marked. They do not, however, in these peculiarities, in any degree approach it.

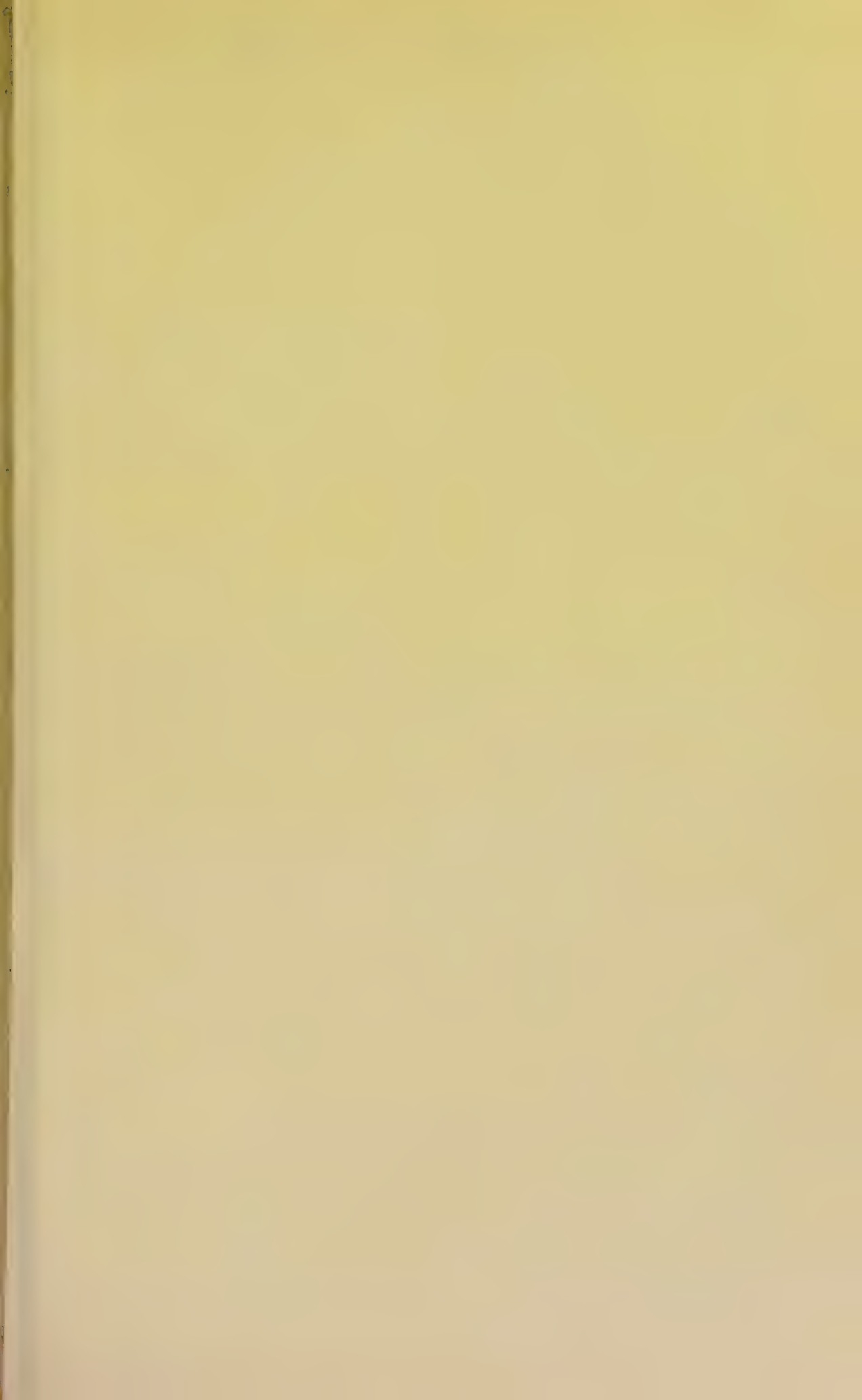
Putting aside the hypothesis of rickets, in any correct use of that term, it may yet perhaps be admitted as somewhat probable that the conditions presented by this skeleton were due to some form of disease which occurred either during intra-uterine life or early infancy. It is perhaps not an improbable conjecture that the influence in question was exerted through the nervous system, and that the brute-like development of the bones of the extremities which it permitted was coincident with some correlated defect in the perfection of the brain itself. In the entire absence, however, of any history of the infancy of the subject it is not possible to go further than the conjecture.

The skeleton is clearly to be assigned to the category of short-limbed dwarfs. The term true dwarf has been claimed for those cases in which the limbs, head, and trunk are all proportionately small. In another, and, I think, a large group, we have cases in which simply the lower extremities are short, the trunk, head, and upper extremities being of normal development. Lastly, in the third and most remarkable group, to which this case must be

assigned, all four limbs are dwarfed, without any obvious implication of the spinal column and skull.

Amongst other instances of dwarfing recorded in the 'Transactions' of this Society I find one by the late Mr. Canton in 1860. It was that of a "true dwarf," the proportions of the limbs, head, and trunk being preserved. There was no evidence of rickets, and it is stated, as in the Norwich case, that the tuberosities, grooves, &c., were unusually well marked. The height of the individual is unfortunately not stated, but the dwarfing of the limbs did not approach that of the Norwich dwarf, for the humerus measured eleven and a half inches and the femur ten inches, whilst in the latter the humerus measured six and a half inches and the femur eight inches. Dr. Robert Barnes, in our Volume for 1848, has also recorded the particulars of a dwarf. It was apparently a case of arrested development in connection with chronic hydrocephalus. The child was nine years old, and his height only twenty-eight inches. As the child was still living we have no special information as to the peculiarities of the bones.

I believe it is very unusual for anything very noteworthy to occur in reference to dwarfing in the lower animals. Our museums do not contain skeletons of dwarf dogs, horses, or cattle, nor do I know of many records of such in life. Darwin has recorded the occurrence of short-limbed sheep, of which the breed was purposely preserved, as they possessed an advantageous disability in respect to getting over fences. I have recently had an opportunity for watching a young dog in which, during several months, it seemed likely that we were going to have a very remarkable example of dwarfing. This dog when a puppy from a month to two months old suffered from what was apparently general rheumatism. After he had shaken this off he presented for several months a very comical appearance of being "all head," and his limbs were so short that he could only just walk; his dentition was also much delayed. When he was about six months old, however, his limbs began to grow, and at the time of his death, a few months later, of distemper, his dwarfdom had ceased to be conspicuous. There could be no doubt that in this case a severe infantile illness, rickets or rheumatism, or both, was the cause of the arrest of growth.





EXPLANATION OF PLATE XIX.

Illustrating Mr. Hutchinson's description of the Humerus and Forearm of the Norwich Dwarf. (Page 7.)

(From photographs.)

PRESIDENTIAL ADDRESS

AT THE

NEUROLOGICAL SOCIETY.

By JONATHAN HUTCHINSON.



PRESIDENTIAL ADDRESS

AT THE NEUROLOGICAL SOCIETY,

January 28th, 1889.

GENTLEMEN,—In thanking you for the unmerited honour of election as President of the Neurological Society, I may say at once that I think that, to some extent, I understand your intention. In reference to special knowledge of the nervous system and its diseases I have but a very poor claim indeed to eminence; nor will I for a moment flatter myself with the belief that you intend to imply it. What you do intend is, I believe, this, that you do not wish your speciality to be a narrow one, and that you have some hope that I may be able to help in reference to general surgery and some special branches, such as affections of the skin and of the eye. Let me assure you that I highly appreciate your compliment, taken in this sense, and that I shall do my best during my year of office to assist what I believe are your objects, trusting meanwhile to your indulgence for my shortcomings as to special neurology. I am informed that it is at once my duty and privilege, as your newly elected President, to deliver an address, and in fulfilment of it I purpose to ask your attention this evening to certain observations of the kind to which I have just referred, in which the surgeon, the dermatologist, and the neurologist need mutual help.

On Retention of Urine after Operations.

To begin with, I will ask your attention to certain remarkable facts in reference to derangement of the nerve supply to the bladder and rectum, following surgical operations, and will commence with a brief narrative.

A healthy man of forty-two was operated upon for piles. The operation was done under ether, at two o'clock one

afternoon, and it consisted in using a crushing clamp, upon which the pile was cut off with scissors. It was not done with any unusual freedom as regards removal of the parts. At midnight of the same day the catheter was necessary, and a large quantity of urine was drawn off. During the next twelve days which followed, the catheter was used three times a day by the surgeon in attendance, and after this the patient continued the use of the instrument himself for another month (indeed, more or less frequently ever afterwards). Urine always dribbled away if any considerable accumulation was permitted, and the patient believed that he always wet his bed, more or less, from the day after the operation. On the second or third day after the operation an aperient was given, and he was warned that the first action of the bowels would probably be attended with a good deal of pain. The dose, however, acted well, without any pain whatever, possibly without any sensation. It may be said, indeed, briefly, that he had no pain of any kind after the operation, neither in the wound itself, nor from the use of the catheter, nor in the action of the bowels. The parts were healed by the end of a fortnight.

Permanent incontinence, both of fæces and urine, with entire loss of sensation in the passages concerned, resulted from the operation just described. On account of this most distressing condition, I was consulted six months later. The above particulars were given me by the patient. I found the orifice of the anus quite lax, there not being the slightest appreciable action of the sphincter. I was told that if the motions were constipated the lower bowel seemed not to make the slightest effort at expulsion, and it was necessary to use an enema, or even to extract the hardened masses by aid of the finger or spoon. If, on the contrary, the bowels were relaxed, then the motion would run away without the slightest consciousness on the part of the patient. The fæces often escaped from him two or three times in the night, and usually he did not know that the motion had passed till he felt it against his thigh. The condition of the bladder and its sphincters seemed to be precisely similar. He was accustomed to use the catheter three times in the twenty-four hours, and said that he had no sensation in the urethra. This measure did not, how-

ever, prevent the urine from dribbling away, and day and night he was constantly wet. He had never since the operation been able to void a stream of urine, unless indeed occasionally in a feeble manner, with great forcing. The skin about the anus and adjacent part of buttocks was, he said, more or less numb, though not absolutely devoid of feeling. There had been pain in the coccyx, and pain across the sacrum and hips ; this, however, he referred to sciatica, from which he had suffered before the operation and on both sides. He had never had any special trouble with the bowels or bladder before the operation. The tendency to piles was in his family, several of his brothers having had them. He had not himself had much pain or bleeding from the piles before the operation.

If we inquire as to the precise nervous lesion by which the distressing conditions above described have been brought about, the first suggestion will probably be that the effect of the crushing was to set up a form of neuritis which ascended to the spinal nuclei, and there destroying the cells, abolished the function of both the sensory and motor twigs. The man's evidence, however, seems tolerably conclusive that the influence of the operation, whatever it was, took effect almost immediately. No interval was allowed for the development of any form of ordinary neuritis. He had retention of urine immediately, and he did not feel the introduction of the catheter. A still more important piece of evidence is that he did not feel the passage of the fæces on the occasion of the first action of the bowels.

As an example of the association of failure of nerve-supply to the urethra and anus simultaneously we have in this case nothing unusual. For, as all well know, these passages and their muscles are supplied by the same nerve, and it is a matter of ordinary experience to find them involved together. It is quite possible that we ought in this case to attach some importance to the fact that the man had frequently suffered before the operation from sciatica. He had, he said, had sciatica on both sides, which is not very common, and he added that during his sciatica he often felt more or less numb on the buttocks. It may be that these attacks had left his nerve-centres more than usually liable to take on disease.

In connection with the above case let us revert to the more ordinary facts as regards retention of urine when it occurs after operations. As you well know, it is very common. It is common after tying piles, or indeed after any operation, in either sex, involving any part near to the bladder. It is not necessary that the operation should involve the anus or perinæum, for the same thing may happen after an amputation of the thigh or an excision of the breast. I have very seldom seen it after operations on the upper extremity or on the head. The most troublesome example of it which I have recently had under observation occurred in a young gentleman for whom I had tied a large varicocele. We had to use the catheter for a fortnight, but he never had any incontinence. I was much impressed in this case by the complete demonstration that the expulsive power of the bladder was much diminished. Most certainly it was not an example of irritative spasmodic retention. There was not the slightest difficulty in introducing the catheter; but, when it was in, the urine was not ejected in a forcible stream, but simply dribbled away; and as the patient was on his back, we had to compress the bladder with the hands, and to ask him to use his abdominal muscles in order to empty the viscus. In this instance I believe that the bladder remained weak for some time after the patient recovered, and probably we had a near escape of permanent ill-consequences. I do not remember that anything was noted concerning the anus or bowels, excepting constipation. Cases in which I have had to use the catheter for a day or two after operations have, of course, been numerous. But I have had none which caused so much anxiety as this one. I believe that I have frequently noticed that the expulsive power was weak, and I feel quite sure that it is the fact that there is never in any of these cases any evidence of spasm of the sphincter, and that the use of the catheter is always exceptionally easy and painless.

On Dry Mouth (Xerostomia) and Allied Conditions.

Permit me next to ask your attention to certain clinical facts as regards the nerve supply to the mouth and its influence on the secretion of saliva. For this purpose we may take the three

large salivary glands together, and may probably associate with them all other secretory structures in the mouth. I do this on the strength of certain well-known facts of daily experience, and others, of a much more rare kind, which are met with as the results of disease. We all know that certain conditions of nerve disturbance, such as fear, and above all what is known as "nervousness," may bring on a temporary condition of dry mouth. The dryness is not limited to one part, and certainly does not concern the large salivary glands only; every part of the mouth is dry, a condition which is implied in the expression, "The tongue clave to the roof of the mouth." A very peculiar class of cases, to which I have just adverted, and to which I shall presently allude in more detail, are characterised by this condition of dryness becoming permanent, and lasting, it may be, for several years. Both these classes of facts place it, I think you will admit, beyond the possibility of doubt that through the influence of the nervous system the secretion of the salivary glands may be absolutely arrested. There is no organic disease of the glands whatever, nor any permanent reduction in their blood-supply, due to disease of their vessels. All that we observe is brought about through the nervous system alone, and I submit that we here have an opportunity for the investigation of the influence of nerves on secretions which it is well worth our while to make the most of. It is probably a fact that all that is true of the salivary secretion is true also, with certain modifications, of that of the pancreas, of the liver, of the intestinal glands, the kidneys, the prostate, and the testes. In addition to the structures which I have named, I ought no doubt also to mention the lachrymal glands and conjunctiva, the mucous membrane of the nose and throat, and lastly, and perhaps the most important of all, the skin itself. The task to which I invite you is the study of the possible extent to which the nervous system may modify or control the glandular secretions from these various parts and organs, and the investigation of the special forms and varieties of symptoms produced by this nervous influence. Let us try to observe in detail the phenomena of sweating, of the flow of saliva, of the flow of tears, of the secretion of milk, etc., remembering that the knowledge which we

thus obtain is in all probability applicable to the explanation of a large class of diseases of the abdominal viscera which are hidden from our sight. It will be applicable, indeed, to almost the whole of the very common maladies to which we are accustomed to apply the term functional, in contradistinction to that of organic. When we use this word we, indeed, almost imply the same thing as if we said "distributed through the influence of the nervous system."

Some of the most interesting of the phenomena in question we can almost all of us study on our own persons. We all know what it is to break into what is known as "a cold sweat," but it is possible we have not all of us troubled ourselves to think out the problem of perspiration accompanied with a sense of chilliness. A cold sweat is clearly something very different from perspiration induced by exercise or by exposure to external heat, and it is perhaps one of the best proofs which we could give that glandular secretion is influenced by causes quite apart from blood-supply. In the phenomenon of blushing we have a rapid and sudden dilatation of capillaries under nervous influence, without any increase in gland action. In the cold sweat we have increase of gland action without any capillary dilatation, it may be even with diminished blood-supply. During the occurrence of the latter the subject is usually pale, and the reason that the skin feels cold is probably that it is temporarily deprived to some extent of its blood.

The production of a cold sweat is, I should suppose, most easily brought about by an extremely sudden consciousness of danger. I have studied it in my own person on several occasions, when I have been suddenly and quite unexpectedly for a moment in fear of falling from a height. Just the last step in mounting a precipice, for instance, may, if attended by a sudden and definite increase of risk, cause you to break out, on reaching the summit, into a perspiration which is attended by a most disagreeable feeling of coldness, and perhaps a slight approach to faintness. The increased action of the skin-glands is not continued long; it covers you with moisture for a few minutes, and then wholly ceases, but it is some time before the balance of the nerve- and blood-supply is

restored, and you may probably feel more or less uncomfortable for an hour or two afterwards. The slight quivering of the muscles, and feeling as if the limbs would fail to support you, are probably evidences that there occurs during the cold sweat a diminution of blood-supply, either from feeble action of the heart or from arterial spasm. A cold sweat is therefore an example of a derangement of the nervous system produced by emotion and affecting the viscera. Who shall say that its influence is confined to the viscera which are imbedded in the skin? May it not extend also to the larger and more important ones, already mentioned, which are hidden from our sight in the abdominal cavity? May not many of the phenomena of temporary stomach- and liver-disturbance, sudden sickness, loss of appetite, etc., be of precisely the same class?

In instancing the cold perspiration as induced by emotion, I mention, of course, only one of its causes. In states of great exhaustion, as in the late stages of phthisis, cold perspirations are very common, and a cause of much distress. So, too, in the death agony there is often profuse perspiration, but under these circumstances it is not always cold, nor is it transitory, as when induced by emotion. Probably in all persons in whom cold sweats are easily induced there is some defect in tone. The better the tone of the subject's system, and by this expression is probably meant the vigour of the nervous system, the less easy it is to produce a derangement of nerve function such as we here witness. It is possible also, as is well known, by the use of certain agents introduced into the blood, to entirely prevent such gland action. Thus, atropine will prevent night sweats, and it will prevent also the secretion of saliva and of milk, whilst it does not diminish the vascular supply. Its action may be demonstrated experimentally in the cat's foot. Cats perspire only on the hairless pad of the foot, and if the divided sciatic nerve be stimulated by the interrupted current a profuse sweating in this region may be induced. Physiologists who detail this experiment* tell us that the perspiration occurs even when the crural artery has been

* See Foster, page 389.

occluded and increase of supply of blood thus made impossible ; and on the other hand, that the injection of atropine will entirely prevent it.

These statements may suffice to remind us that the influence of the nervous system in secretion is by no means simple, but rather, full of intricacy. It would be beyond my present purpose to attempt to recapitulate all that is known or conjectured respecting it. I will now pass to the clinical consideration of the disorder known as "dry mouth."

The first case of "dry mouth" which has, I believe, ever attracted professional attention was brought under my notice by Dr. Cotman of the Minorities. Its subject was a widow woman of about sixty. After an emotional shock which involved a disappointment to her affections, her entire mouth became dry ; and that I may shorten the story, I will simply add that it has remained so ever since and under all variations of external circumstances. I was exceedingly interested in the case, having never seen anything so extreme before. I showed the patient to our first President, Dr. Hughlings Jackson, and subsequently brought her before a meeting of this Society. You will understand that what I mean by "dry mouth" is quite distinct from what is spoken of as *pharyngitis sicca*, and that it is wholly unattended by inflammation. It is simply a cessation of gland-action on the part of all the salivary and mucous structures in the mouth, and is denoted by dryness of palate, cheeks, lips, and tongue. The pharynx is more or less affected, but the nostrils and the conjunctiva escape. Thus, you will observe it is the state of things become permanent which is so often seen as a transitory state during nervous fear, and it may be taken, I should suppose, to prove that there is a centre which controls the secretion of fluids into the mouth, which does really control them absolutely, and which is quite distinct from that which regulates the flow of tears and the moisture of the conjunctiva and nasal passages. We have a sort of experiment performed for us, whether in anticipation of physiologists or in confirmation of their knowledge I will not venture to say, but certainly of the greatest possible interest.

I have already mentioned some of the experiments on the feet

of cats, but there is yet another which I must trouble you with, since it affords the strongest possible evidence in support of the belief just referred to. If a cat in which one sciatic nerve has been cut be exposed in a closed room to a high temperature it will break out into perspiration on three of its feet only. The foot which has been deprived of nerve influence will remain dry. Could we ask for more conclusive proof of the direct power of the nerve centres upon the secretion of sweat? The high temperature acting locally upon the paralysed part is ineffectual to induce gland action. This latter depends for its exciting influence upon the nervous system. It is thus clear that heat does not act directly in causing perspiration, although at the same time we must admit it to an important place as a favouring influence.

To return to our cases of dry mouth, which I may say it is now proposed to name *Xerostomia*, and in which it is, I think, clear that the condition depends upon changes, probably of a permanent kind, in the central nucleus which dominates the secretion of saliva. As I have already said, my first case was made public only two years ago. In the brief interval which has elapsed nearly ten others have been recorded. The second of these was brought before the Clinical Society by Dr. Hadden, of St. Thomas's, and, I need not say, very carefully recorded. The cases are all just alike, and in no single one has a cure been obtained. All the patients have been women, and nearly all past middle life. In a second case which came under my own observation the lady had suffered from dry mouth continuously for six years, and was still in good health. Her whole mouth was absolutely dry when I saw her; if I were to use the common standard of comparison, "as dry as a stick," I should scarcely exaggerate, for she could hardly make her speech understood, owing to the arid condition of her tongue and palate.

In most of these cases no plausible conjecture could be given as to the predisposing or exciting causes. The women were in good health when the condition arose, and remained so in spite of it. In most of them it began rather suddenly, and soon reached its complete state, but we have no very accurate observations as to its initial period. In most of them, although the mouth is under all

conditions which might be called quite dry, yet the state becomes aggravated under emotional excitement. The cases are sufficiently numerous, and the patients have been under observation for sufficiently long periods, to justify us in the belief that the symptom is not one implying aggressive disease, for no other form of paralysis or other ailment has been developed in any one.

You will naturally ask as to the effects of drug treatment, more especially as to those sialagogues which are believed to act directly upon the gland-cells without the intervention of nerves or blood-vessels. Chief amongst these is, I believe, jaborandi, and its essential principle pilocarpin. This remedy has been tried in most of the cases, and no one has recorded any benefit from its use excepting Dr. Hadden. Dr. Hadden's evidence is most valuable, to the effect that by the continuous use of tincture of jaborandi in half-drachm doses the mouth is made more moist, and the patient much more comfortable. In one of my patients this remedy has been two or three times tried, at long intervals, and always with the result that the patient perspires freely, and feels very weak, whilst there is still no return of the saliva. She always declines to continue it. The difference between Dr. Hadden's case and mine may possibly depend upon the circumstance that his case was of much more recent development. After the condition has lasted for years, as in both my cases, it may easily be the fact that changes occur in the non-used glands which incapacitate them for resumption of function even under the most direct stimulation.

Before leaving the subject we may recall the fact that saliva is not always quite the same fluid, and that especially it may contain more ptyalin and less water in relation with different kinds of excitement. If we remember this fact we shall admit that it is possible that in these cases the arrest may be more complete as regards the watery parts than the solids.

There is a condition, the opposite to dry mouth, in which a great excess of saliva is secreted, or the secretion continues at unwonted and inconvenient times. This when it results from the action of mercury is known as "ptyalism," and when independently of it as "spontaneous ptyalism." Perhaps, in order to have a name

which shall be a definite converse of *Xerostomia* we might more suitably name it *Hygrostomia*, and leave the word *ptyalism* for cases attended with general stomatitis, as after the use of mercury.

Functional and temporary conditions of *xerostomia* and *hygrostomia* are very commonly seen in the same person, and evince only that the nervous balance is not strongly sustained. They are neither of them common in young persons, and in the more typical examples are seen with commencing senility. I believe that a very wet mouth is in some instances an early symptom of general paralysis and other aggressive disorders of the nervous system, but I have known those who suffered from it occasionally for many years, and yet developed nothing of serious consequence. I do not think that I have ever seen an example of profuse and permanent *hygrostomia* which might be placed side by side with those of *xerostomia* which I have adduced. Two which approach to it have, however, been under my notice, and in each there have since developed other indications of nervous disease.

On Skin Diseases of Nerve Origin.

We will next, with your permission, pass on to consider certain skin diseases which present problems of great interest for the neurologist.

A few general observations may first be ventured as to the best means of distinguishing those diseases of the skin which are of neurotic origin from others. It is very important that we should, as far as possible, escape from vagueness in the use of words in this matter, and not speak of a disease as "a trophi-neurosis" or "due to vaso-motor disturbance" unless we have substantial grounds for such an opinion. The nervous system takes its share, and cannot help doing so, in all diseases. It is scarcely possible to name any pathological change whatever which does not implicate nerves, and nerves once implicated are certain in some way to originate reflex disturbances, and take their part in the general commotion. It is not, however, in this vague sense that it is desirable to say of any skin disease that it is of neurotic origin. We ought rather to reserve that term for diseases which begin primarily in nerves, or are at the starting-point produced by them. These we may, per-

haps, classify under three heads—first, those in which some part of the nervous system is itself structurally disordered, and in which organic changes passing along nerve fibres extend to and involve the end-organs in the skin. A second group may include those in which local disturbances of nerve function induce in the skin secondary changes, for the most part dependent on altered supply of blood; and lastly, we have a group in which there is no local nervous change, whether of structure or of function, but in which a general lowering of what may be called “nerve tone” permits the development of pathological processes which would not have been possible if it had been well preserved. Under the first group (in which the nerve structures of the skin are themselves involved in organic disease) we have as our chief examples herpes and localised morphœa or scleroderma. In the next group, in which the nerves induce vaso-motor disturbance, we find our chief example in Raynaud’s disease; whilst in acne and its allies we have examples of local disorders of nutrition made possible by a general lowering or disorder of the nerve tone. I do not know whether we can fairly group with the latter class such disorders as urticaria and certain forms of localised gangrene, concerning which our knowledge is as yet but vague. I may say at once that it is not my purpose in the present address to enter into any discussion on the vexed question of “trophic nerves,” and I shall designedly leave aside the disturbances in the nutrition of the skin which have been observed in connection with division of nerve trunks. These conditions, with which the names of Weir Mitchell and Paget are so honourably associated, are of very great interest, but for this evening we shall have quite enough if we confine ourselves to nerve disorders of the skin not of traumatic origin.

If we seek for rules by which to diagnose the affections which belong to the first group, and are produced by disease of the nerves themselves, I think the following may be established:—

First, a skin disease which is dependent on disease of the nerves of the skin will be distributed according to their distribution. It will not occur in round patches, nor in oval ones, nor in streaks, but will be arranged according to the branching

distribution of the nerve filaments themselves. It will be paniculate or corymbiform. This is, I believe, invariable, and to my mind it is conclusive against the notion that such a disease as alopecia areata, which occurs in round patches, can be of nervous origin.

Secondly, a skin disease of nerve origin will develop no power of infecting adjacent structures. Its patches will not be serpiginous; they will not creep at their edges, nor will they produce any satellites near to them. Respecting all the common forms of skin disease, we may assert that they always show, in greater or less vigour, the power to infect adjacent tissues. This is true of lupus, psoriasis, eczema, leprosy, and numberless others. In proportion to its definiteness may we feel confident in our belief that the disease so characterised is not primarily neurotic. In herpes and scleroderma there is no tendency to spreading.

Thirdly, a feature which always, I think, characterises neurotic skin diseases, and which is perhaps a necessary consequence of what has just been said, is that they develop themselves in the first instance to their full extent, just as an attack of infantile paralysis does. The thing is done by a sort of *coup*, and it is over, and we may be sure that its results when once declared will not undergo increase. This feature is very marked in herpes and scleroderma, which are our types of disease in the skin due to nerves.

If we attempt to apply these rules we shall find, as already said, that they at once exclude all diseases of which the patches are either circular or made up of circles. Nothing with which we are acquainted in the nervous system or in the distribution of nerves could account for a round patch any more than for a square one. Nor could it explain anything which is gyrate in its distribution, for gyrations are usually made up of circles which have coalesced. The disease known as leucoderma, for instance, in which the patches are always either circular or irregular with crescentic borders, may be with great confidence, from that feature only, excluded from the neurotic group; and I have already hinted the same opinion respecting the disease known as alopecia areata, and which is by many dermatologists claimed as a disease of the

nervous system. One might, indeed, almost say that anything that deserves the name of areate or circumscribed, adjectives which ought to imply great definiteness in the boundary-line, is, from that feature alone, proved to be other than neurotic. With regret, I observe that some recent dermatologists speak of a form of morphœa as "circumscribed," in contradistinction with those which are diffuse and those which occur in "streaks." The terms are, I think, not well chosen, for, if carefully examined, it will be found that the changes of morphœa are never either accurately "circumscribed" nor, on the other hand, are they limited to "streaks." I quite admit that the more conspicuous lesion is in some cases an oval patch, and in others a streak, but if carefully examined it will, I think, always be found that there are other lesions near to the margin of the patch or streak, which destroy its character as such. Further, let me suggest that the patches, when apparently most definitely circumscribed, are due to the coalescence of a group of spots, and that these spots are never arranged in the form of a circle, nor even of a well-drawn oval. The patch is invariably irregular in outline, and by this irregularity proves its dependence upon a nerve which has divided into filaments.

Concerning herpes and its connection with nerves, much has been written of late years, and many facts are now so generally accepted that I shall not feel it necessary to trouble you with their recapitulation. Every one now admits that herpes zoster is produced by nerve disturbance, that it is, in fact, an eruption denoting neuritis. Of late years not a few observers have had opportunities of examining the nerve structures implicated, and have demonstrated the occurrence of nerve lesions, in some cases inflammatory, and in some hæmorrhagic. Our knowledge is, perhaps, as yet, scarcely sufficiently advanced in this direction to permit us to put much confidence in it, and in what I have to say this evening I shall prefer to keep to the clinical aspect of the subject rather than to detail the facts as to its pathological anatomy.

It is well known that herpes zoster is not usually recurrent, and that few suffer from it a second time, whilst there are other forms of herpes of which the prominent feature is recurrence, and which recur so frequently that the patient is scarcely ever free from them.

Another fact may be added to this statement ; it is this, that when common zoster does occur a second time to the same individual it never affects exactly the same part, whilst in recurrent herpes exactly the same part is attacked over and over again. May we infer from this that the kind of neuritis which produces herpes zoster usually disorganises the nerve affected, making it impossible for it to be so affected a second time, whilst in the recurring forms of herpes the affection is less severe and more nearly allied to what might be called functional disturbance ? This suggestion is borne out by the fact that we very frequently observe that common zoster may leave deep scars, whilst recurrent herpes never leaves any scar at all. The portrait of herpes zoster published by the New Sydenham Society, and showing the eruption on the side of a boy's chest, is of much interest in reference to what I have just said, for it shows the deep scars left by an eruption of the same kind some years before. The second eruption is close above, but not upon the territory previously affected. Clearly it has been distributed by a different nerve. Many years ago I published the observation that the medicinal use of arsenic very frequently causes herpes zoster. To this I may now add that the recurrent herpes can in the most definite manner be prevented by the use of arsenic. I have given arsenic for this purpose now during many years, to many persons, and only in a very few instances have I had any exception to the statement that the tendency to the herpes ceased as soon as the arsenic was begun, and was absent as long as it was used. In one interesting case the drug which cured the recurrent herpes produced in the same patient a sharp attack of zoster. Certain anomalous cases of recurrent herpes, occurring usually on the buttocks or thighs, are met with in association with other nervous disorders, such, for instance, as locomotor ataxy. Of these our distinguished vice-president Dr. Buzzard has recorded a remarkable example. Recurrent herpes, as is well known, is far less common on the skin of the limbs and trunk than it is on certain mucous surfaces and the skin in close contiguity with them. On the lips and on the genitals it is very common ; everywhere else it is rare.

That in these positions it has something to do with the abundance of their vascular supply, and the ease with which disturbances of that supply occur, is highly probable. We have no recurrent herpes at the navel or at the anus, nor is it common either at the nostrils or at the eyelids. When it occurs on the lips I believe that it is almost always in association with something in the nature of a rigor, and we all know how quickly and definitely the supply of blood to the lips is altered when this occurs. I do not mean that a definite rigor precedes every attack of herpes on the lips ; but I believe that if patients were careful to note their symptoms, they would observe that some kind of chilliness had always preceded it. Of this I am quite sure, that the severity of the herpes in pneumonia after catheter-passing and in ague is usually proportionate to the severity of the rigor. May we venture to conjecture that it is to the temporarily exsanguined condition of the nerve end-organs during the attack of arterial spasm which causes the rigor that the outbreak of the herpes is due? The latter would then be an inflammation consequent upon temporary deprivation of blood, succeeded after an interval by turgescence.

Many of the anomalous forms of herpes, perhaps almost all those which I have alluded to as occurring on the skin of the thighs and buttocks, which are recurrent, occur to those who have had syphilis, and are probably due to a form of neuritis in association with that disease. We must, I think, admit that the facts relating to recurring herpes denote possibilities as regards peripheral neuritis which we are far from being able to understand or explain. We know nothing in connection with other diseases of a neuritis which can recover and recur with the rapidity which we here witness.

I cannot leave the subject of Herpes without alluding to a disease which is characterised by vesicles occurring over almost the whole of the body, but arranged somewhat on the corymbiform or racemose pattern. This is a disease originally described by Dr. Duhring, of Philadelphia, under the name of "dermatitis herpetiformis." It is of rare occurrence, and some of its examples belong definitely to syphilis. In a very remarkable instance of it

which was not syphilitic, and of which I am fortunately able to show you an excellent portrait, the disease was in a most remarkable manner under the control of arsenic.

It is possible that in some skin disorders which are certainly of blood origin a peripheral neuritis may be one stage in their evolution. We owe to Hebra the observation, which I think all will confirm, that the eruption of variola itself is very frequently corymbiform in arrangement, and looks like herpes. In the fact that its pustules are occasionally gangrenous, and that they usually leave deep scars, we have further suggestions as to its being possibly dependent on a neuritis of end-organs.

Dr. Stephen Mackenzie has recently drawn our attention to a fact which I can fully confirm, that the eruption in lichen ruber and some other diseases is sometimes arranged on the herpetic or corymbiform pattern. It is quite possible that we do not as yet at all appreciate the extent to which the influence of peripheral neuritis goes in modifying diseases of the skin.

On Morphœa, Raynaud's Disease, etc.

In morphœa, or localised scleroderma, we have a disease which in its evolution and arrangement is usually exactly like herpes zoster. Like herpes, further, it shows a tendency to spontaneous recovery after a certain duration, and it very rarely recurs in the same individual. Like herpes, it is usually an affection of the skin only, but in exceptional cases may involve the deeper parts as well. Like herpes, it may occur at any age (excepting infancy), but prefers the young. It is unlike herpes in that it is more severe in young persons than it is in the aged, but this severity is manifested chiefly by its proneness in the young to affect the deeper structures. It differs most remarkably from herpes in the duration of its stages; its periods of evolution, persistence, and decline being measured by months or years instead of days. Like herpes, it is wholly unconnected with any peculiarity in the health or state of the patient. It occurs, so far as our knowledge extends, quite causelessly. There can be no doubt respecting it, any more than as to herpes, that its lesions are distributed in the course of nerves, and that they are primarily produced by nerves.

The disease of which I am speaking is that which was originally described by Thierial, which next took the name of Addison's keloid, and concerning which, under the latter name, the physicians of Guy's Hospital—Dr. Addison, Sir William Gull, and Dr. Hilton Fagge—have in turn contributed so much valuable information.

Having compared Addison's keloid (or *morphœa lardacea*, or localised scleroderma) with herpes, it is next necessary to describe in some little detail the differences in the forms of inflammation of the skin which occur in each.

In herpes zoster an inflammatory infiltration occurs over or in the end-organs of cutaneous nerves, which is attended by serous effusion and the production of sub-epidermic vesicles, which rapidly pass through their stages and as a rule leave scars. These vesicles often coalesce in little irregular groups, or, it may be, in one large patch. It is not at all an infrequent occurrence to see definite gangrene of the portions of skin involved. It may be in minute separate spots, or it may be, in consequence of coalescence, in one large patch. In *morphœa* there is just the same tendency to the production of corymbiform groups of spots, but the spots do not develope into vesicles, but end by solid infiltration, which has a white appearance and has been compared to ivory or lard. The tendency to coalescence is yet more marked than it is in zoster, and in this way very usually an oval or streak-like patch—it may be of considerable size—is produced. However large this patch may be, there are usually to be seen near to its edge some little isolated specks of lardaceous deposit, which are analogous to the outlying vesicles of herpes, and prove the original tendency to corymbiform arrangement. A feature in which herpes and *morphœa* are most conspicuously alike is, that when once developed, they do not spread at their edges. I am aware that some observers have doubted the correctness of my assertion on this point as regards *morphœa*, but as the result of careful observation of many cases, I cannot feel the least doubt that it is a fact, and a very important one. We must make allowance for the slowness with which the disease is evolved, and not take its measure before it is complete. This being done, I feel sure that all will admit that *morphœa* possesses no serpiginous quality. The inflammatory

infiltration which it produces is not capable of infecting adjacent parts, and it ends where it was originally mapped out by the dis-



Morphea with Muscular Atrophy. (From a Photograph given me by Mr. Symson, of Lincoln.)

tribution of the nerves which are involved. Unlike herpes, morphea is not a painful disease; its patches are, indeed, neither

tender nor, for the most part, much deficient in sensation, and from this fact we are perhaps justified in inferring that vaso-motor filaments, rather than sensory ones, are those by which it is distributed. When the disease begins in adults I believe there is very seldom any evidence of implication of muscles or of parts deeper than the skin. The younger the patient the greater is the risk that the muscles will suffer, and that with them the bones and fasciæ may also undergo changes. These changes may result in very definite arrest of growth. The muscular affection is, I believe, not primary. I do not know of any proof of failure of the power of a muscle in the first instance, and the changes which ensue are such as might have been expected, if we are permitted to assume that they are consequent on deranged vaso-motor influence. Many years ago I ventured the suggestion that infantile paralysis, herpes zoster, and scleroderma are diseases which should be placed in one group, in reference to the probable nature of their nerve-cause. Just as the first of these is sometimes spoken of as "essential paralysis," that is, I suppose, as paralysis and nothing more—paralysis of muscles which begins and ends as such, with no tendency to spread—so in herpes do we appear to have an essential affection of a sensory nerve, and in scleroderma of a vaso-motor one. In all three diseases the area to be affected is known from the beginning. In all there is a tendency to reparation to a certain extent, but in none is the reparation ever complete. In none, as a rule, do second attacks occur. Respecting none do we know anything as regards predisposing influences or means of cure.

In the drawings and photographs which I exhibit I have illustrated the distribution of the morphœa patches, and the character of the secondary atrophy, or rather want of growth. Several of these portraits show the condition known as hemi-atrophy of the face, a state which I feel justified in associating with morphœa, because it exhibits conditions precisely similar to those which we see in the limbs after the latter disease, and because it is often seen in conjunction with morphœa patches on the limbs of the same patient. This last fact was especially well illustrated in the case of a woman named Nicolls, whose portrait is before you,

and who at different stages of her disease came under the ob-



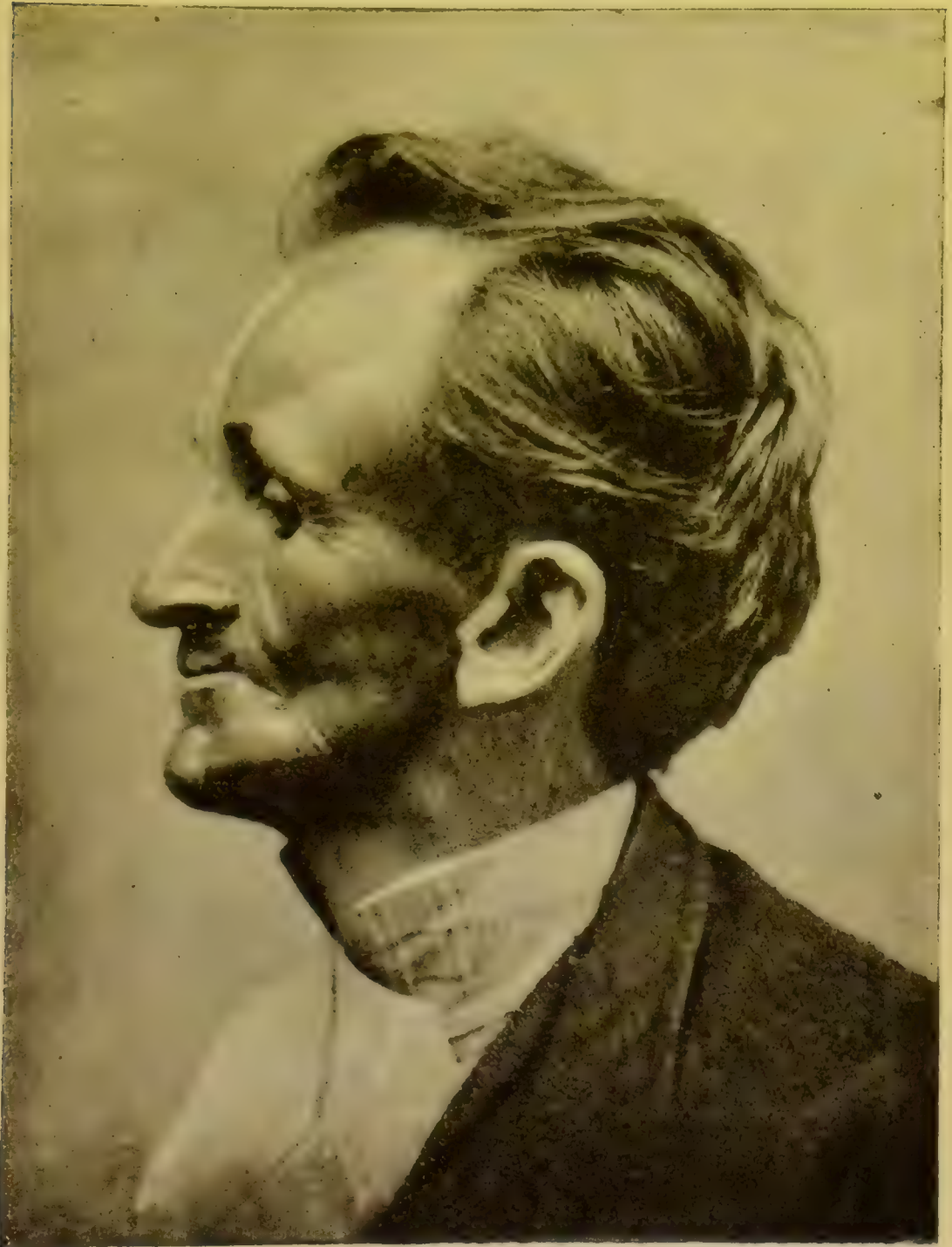
Hemiatrophy of Face after Morphœa.

servation of Dr. Addison, Dr. Hilton Fagge, and myself. This woman had one leg amputated in consequence of the deformity

produced by morphœa, her limb being, I believe, much in the condition shown in the photograph which I produce of another case. The state of the woman's face as regards unilateral atrophy is well shown in the portrait before you. Permit me to repeat that the so-called atrophy is really arrest of growth, and that its severity is usually proportionate to the earliness of the age at which the disease begins.

The problem before us as neurologists is to determine, if we can, the kind of influence which the nerves exert in the production of this curious malady; and our difficulties are exactly the same as they are in the cases of herpes. The slowness with which the phenomena of the disease are evolved, as compared with the rapidity and suddenness with which herpes zoster and infantile paralysis develop themselves, may probably be explained by the fact that what we see in the skin is only secondary to the vaso-motor nerve change, and therefore requires time for its production. Its slowness and long persistence may also well explain the circumstances that in many instances deformity results, with severe permanent changes, and that the recovery is only partial. That there is in the worst cases, however, a tendency to recover may, I think, always be observed, and a large number of the slighter cases recover almost perfectly. An argument in favour of the belief that morphœa is due to an organic disorder of the vaso-motor filaments distributed in association of sensory and vaso-motor nerves, may, I think, be derived from the fact that some of its phenomena closely resemble some of those which occur in Raynaud's disease, concerning which it will be generally admitted that functional disorder of the vaso-motor is their cause. I refer to the tallowy whiteness of the skin involved, a condition to which, when aggravated, the term "lardaceous" in morphœa has been employed. In the condition known as diffuse morphœa, or scleroderma, we have a kind of connecting-link between localised morphœa and Raynaud's disease. In diffuse morphœa conditions very similar to some of those in Raynaud's disease have become permanent, and the difference might appear to be chiefly that while Raynaud's phenomena are due for the most part to

functional disturbance, those in diffuse morphœa depend upon



Hemiatrophy of Face after Morphœa.

organic, and therefore permanent change. In diffuse morphœa the parts affected become pale, ex-sanguined, tallowy, and hard ;

and to such an extent may the shutting off of the blood-supply go that gangrene of the tips of the fingers is not infrequently observed. Diffuse morphœa differs from the localised form in that it is, so far as I am aware, always symmetrical, and for the most part general. It affects the whole face and both upper extremities, usually involving the trunk and lower extremities in a



Hemiatrophy of Face after Morphœa.

far less degree, and sometimes to so small an extent as scarcely to be appreciable. It is a disease, I believe, of adults only, and almost exclusively of the female sex. There can be little doubt that it is an affection of the nervous system, and that it has its origin in central parts in connection with the vaso-motor nerve. There is some reason to believe, as pointed out to me by Dr. Barlow, that cases which begin as Raynaud's disease, and which

are in the first instance intermittent and probably only functional, may slide by gradations into confirmed scleroderma.

It will be convenient at the present juncture that I should say a few words concerning the very interesting disease, or group of diseases, which will probably henceforth be known in connection with the name of Raynaud. Briefly, I think it may be said that in this class we must group everything that is the result of abnormal irritability of the vaso-motor system, with tendency to vascular spasm and temporary occlusion. In connection with this occlusion of arterioles, brought about under some reflex stimulation or inhibition, or both, we have the tallowy white fingers known as *digiti mortui*, and at a later period the lividity from venous turgescence. That the conditions are functional in the first instance in the great majority of cases is proved by the fact that they are for the most part transitory. Their liability to recur and to increase in severity, or even to slide on into organic disease, is probably only an instance of the acquisition of pathological habit. For the neurologist the interest in Raynaud's disease centres in the inquiry as to the manner in which extreme derangements of vaso-motor regulation may be brought about, and further as to how they may best be remedied. In reference to the latter point, it is well known that Raynaud himself believed that he had discovered in the use of the continuous current a method of cure which was far superior to all others.

The lesions which occur in the skin in *Leprosy* are, I believe, never localised by nerves. It may seem a bold assertion to say this, but a close observance of the phenomena will, I believe, convince any one of its truth. The lesions of the nerves in leprosy are secondary to those in the skin, and not the cause of it, at any rate in the majority of cases. A dusky erythema of the skin, which may show crescentic borders, and which destroys, in all probability, the end-organs of the skin involved, is usually the first stage. From these end-organs the disease travels up as a process of ascending neuritis to the nerve trunks, and may reach the parts where those that supply muscles are given off, and thus induce muscular paralysis as well. An able paper, in which this hypothesis is advocated by my friend Dr. Allen Sturge, from cases which

he and I studied together, will be found in our Journal, BRAIN, for 1884. The loss of sensation in the skin which occurs in leprosy is due to two different causes—first, and most commonly, to inflammatory destruction of the end-organs in the skin itself; and secondly, to implication of the nerve trunks. In making these statements I by no means wish to assert incredulity as to there ever being a primary neuritis in leprosy, but only that what I have said is the general rule.

We may next say a few words on the question as to whether eruptions which are arranged in streaks can be claimed from that fact to be of neurotic origin. I may say at once that I do not know of any disorder of the skin which is produced subsequent to birth, which is arranged in streaks, and in which there is any probability whatever that such streaks are due to nerve influence. We may regard such expressions, and they are not very uncommon, as “developed along the course of a nerve trunk,” as implying some forgetfulness of anatomy. A nerve trunk running under the skin on its way to a distant field of distribution can probably exert no influence whatever on the nutrition of the skin which overlies it. It would be as reasonable to suppose that the electric wires influence the houses over which they pass. It is only when the nerve reaches its end-organs, and branches out into filaments, that it can produce evidences of disturbed nutrition. And it is well-nigh impossible that such disturbances should be restricted to the form of a line, or streak, or band. They are certain to be more or less racemose. As I have just hinted in reference to the word “streaks” as used in describing certain forms of morphœa, I do not think that it is strictly applicable, for the changes are never restricted to a streak only, although a broad streak may be the more conspicuous lesion.

There occur, however, a group of cases in which skin-changes are met with in streaks, which are present at the time of birth, and are of the nature of what is called ichthyosis, and which are very difficult indeed of explanation. They are so obviously and conspicuously non-symmetrical that it is impossible to avoid the thought that they must be due to nerve distribution. Thus they may pass in bands, curling

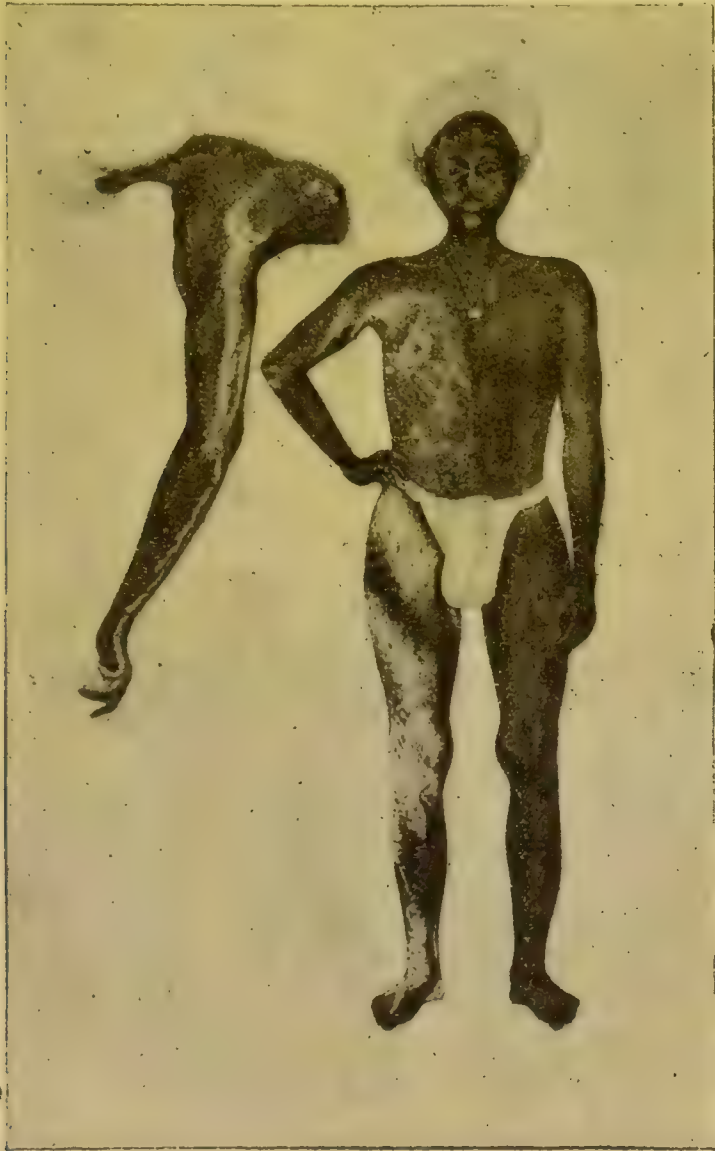
round one side of the chest or abdomen, from the spine to the linea alba, and looking exactly like herpes zoster. So much did this appearance impress me, as no doubt it has done others, that many years ago I made the suggestion that they might be due to some intra-uterine disease analogous to shingles. They are not, however, as a rule, restricted to one half-circle, as in shingles, but may cover half the trunk, or may in some rare cases involve almost the entire trunk. When these streaks of ichthyosis occur on the limbs they usually—indeed, I think always—defy the anatomist to allot them to any special nerves. Thus, a long streak may run, not broader than the little finger, from behind the great trochanter to the external malleolus, or, what is more common, down the inner part of the arm and forearm to the little finger. They do not even overlies any known nerve trunks, nor do they at any part of their course or their termination show any tendency to branch out into filaments. It may be that those skilled in embryology will some time give us an explanation of these remarkable appearances, and it may be that they have to do with something that is transitional in the development of the nervous system. At present I must be content with having drawn your attention to them, and to the fact that they are not so easily explicable as some writers would seem to suppose.

In connection with this subject permit me, Gentlemen, to draw your attention for a few moments to a very remarkable specimen. The skull which I hold in my hand shows evidence of overgrowth of bone, with broad-based exostoses, some very large and some very small, having this peculiarity, that they are all confined to bones of the right side. In the lower jaw the changes come up to the symphysis of the chin. On the palate, again, they come close to the middle line, but are still all on the right side. The same will be found with regard to one on the basilar process, and the same with the bossy masses on the frontal and sphenoid. Not a single bone on the right side has escaped, and not one on the left side has suffered. Such definite unilateral asymmetry cannot possibly be explained by reference to the vascular system. It is due either to some trophic influence of the trigeminal nerve or to some hitherto undiscovered law as regards unilateral growth. I

am very sorry that I cannot tell you anything as to whether these changes were initiated in early life or before birth. I believe that they only became conspicuous in middle age. The skull is that of an aged man, and concerning his early history no facts have been preserved. We have here a sort of converse of what is called the hemi-atrophy of the face, which latter is always attended by hemi-atrophy of the bones also. I have already explained that I believe this hemi-atrophy has been misnamed, since it is not an atrophy in the sense of being a diminution of anything that previously existed, but simply an arrest of growth in an early stage of childhood. That disease, so far as I know the facts, we may believe, without risk of error, to be of the nature of *morphœa*. I do not know of any museum specimen of the bones of the head from one of these cases of hemi-atrophy, but attention having been drawn to the disease of late years, it may be hoped that some zealous observer may before long secure one. The skull which I have shown is, I believe, unique, and I know of no specimen whatever which approaches it in value as showing what is possible in the nature of unilateral disease. I have seen one other case in a living patient in which there was reason to believe that similar conditions existed, though to a much smaller extent.

I have tried to prove that all diseases of the skin which depend for their distribution directly upon nerves must assume a certain definite method in their development, and that they can never be either round or crescentic, or irregularly scattered in their location. It remains, however, to be admitted that the nervous system may take a share in diseases which are by no means in a direct sense dependent upon it, and which are certainly not in any way distributed by its branches. By the term "nervous tone" I suppose that we denote that state of the nerve cells which implies more or less of functional perfection. That man's system is in the best tone in whom the innumerable reflexes needed for healthy nutrition and healthy discharge of function in the various organs are the most nearly perfect. If the tone be lowered the efficiency of the nervous system as a means of regulating the supply of blood according to the special needs of every part, and possibly, if we accept the hypothe-

sis of trophic nerves, as a means of controlling the healthy nutrition of all parts of the body, becomes impaired. In this way it is obvious that a lowering of nerve tone may in some sense permit, or even originate, certain diseases of the skin. I have already alluded to



Congenital White Streaks on the Skin of a Hindoo.

Raynaud's disease as an example of the impairment of such nutrition brought about through the influence of the vaso-motor nerves. But there are yet other disorders, of a less easily explicable character, concerning which I cannot but hold that they come

into a similar category. In some of these the nervous system takes a large share, and in some only a small one, but it is impossible to exclude it. It is to be remembered that lowering of tone is not by any means the same as debility, and is very



Congenital White Streaks on the Skin of a Hindoo.

different from general inanition. The inanition may concern the nerve centres only, and it may be of a very temporary duration. It may be produced by causes which have nothing to do with the food of the individual, nor with the exposure of his body to any

physical cause of disease. This subject is a very large one, and although of the utmost importance in that it mixes itself up with one-half of the constitutional ailments which we observe, I must not venture on the present occasion to say more concerning it. I will, however, just add, by way of illustration, that the best evidence of the effect of disturbances of tone of the nervous system on the body at large is offered us in connection with the sexual system. Nothing more surely and promptly lowers nervous tone than what partakes of the nature of sexual excess or disturbance, and it is well known that a host of local disturbances of nutrition may occur in such connection. Of these the eruptions known as acne are perhaps the commonest. In many women the occurrence of menstruation always produces acne on the face, whilst in many young men other forms of sexual reduction of tone have for their result, in like manner, inflammation of the sebaceous glands on the face and shoulders. These eruptions in many instances have nothing to do with the state of the blood or the general strength, and occur to those who are well fed and in no way debilitated. The nervous system clearly takes a considerable share, although it is probably by no means the sole cause.

I fear, gentlemen, that I have trespassed on your attention at unreasonable length. I fear also that it may be thought that I have been wandering from subject to subject in a somewhat fragmentary manner. It is true that we began with paralysis of the sphincters after the operation for piles, and have ended with acne, having concerned ourselves meanwhile with xerostomia, Raynaud's disease, herpes, and morphœa. There has been, however, I would submit, a close bond of connection in our topics. I have been trying to show that there exist possibilities as regards local disturbances of nerve function with the real nature of which we are as yet but little acquainted. These possibilities, demonstrated beyond all doubt in the disorders which occur in external parts, may be fairly held to be likely to take an important share in those of parts which are concealed from our view. I have not been asking you to take interest in morphœa or zoster for their own sakes, but because they present us most important forms of neuritis which it would never have

occurred to us to conceive as possible did they not reveal them. Who would have conjectured that neuritis would be likely to produce vesicles over the nerve-ends, that it would be a transitory ailment, or that it would sometimes be prone to recur very frequently, unless herpes had proved it all for us? Again, what might have seemed less likely than that another kind of peripheral neuritis should be attended by lardaceous induration of the implicated skin if the phenomena of morphœa did not show it? Do not let us quench our wonder at such strange phenomena by contentedly naming them "tropho-neuroses," but let us seek diligently for detail as to their modes of occurrence and their causes. We shall be rewarded if we do so. The subject of dry mouth is again one, I think, of paramount interest as a demonstration of what is possible in reference to the permanent arrest of gland function through nerves. So, again, of the case of sphincter-paralysis with which I began. That it should be possible for a local or superficial injury such as the crushing of piles to be attended immediately by a central ganglionic lesion (for such I cannot but believe must be the condition), which should produce both anæsthesia and muscular failure in two associated regions, is a fact which ought certainly to open our minds to possibilities in neuro-pathology of which I make bold to believe we have as yet no knowledge. That it is allied to peripheral "neuritis" we may fairly conjecture, but what kind of "neuritis" is it? By its side we may suitably remember that herpes zoster is often, and probably with truth, attributed to blows or other injuries. Indeed, I feel sure that the more carefully we consider the class of cases which I have ventured to bring under your notice to-night in association, the more shall we find that they throw light upon each other. Our great physiologist Michael Foster has told us that it is time that we should give up the word paralysis, and try to name the conditions which it usually denotes by reference to the lesion or local change which causes it. There is much truth in his remark; we must seek for causes, and not be content with names. I doubt much whether any department of neurology offers more attraction for study in this spirit

than that which concerns the neurotic derangements of function and structure in the mucous membranes and skin.

SOME APPENDED NOTES TO THE ABOVE ADDRESS.

In the report of my address at the Neurological Society (given above) I omitted, by a mistake, a not inconsiderable part of what I had to say respecting the means of recognition of neurotic affections of the skin. In now supplying that omission I am not sorry of the opportunity it affords for adding a few additional notes by way of appendix to the lecture.

As regards the features by which neurotic skin affections are usually characterised, I mentioned their racemose form ; secondly the absence of serpiginous tendency ; and thirdly, the fact that they usually occur at one stroke, their first full development being complete. In my lecture (as delivered), in addition to these points, I mentioned the fact that they are never attended by any implication of the lymphatic system or enlargement of glands. This follows as a part of their general and very remarkable feature of non-infectivity. In herpes zoster we never expect to see any implication of the adjacent skin, nor any signs of irritation of the lymphatic vessels or glands, and it is the same with morphœa and all other skin disorders which acknowledge a neurotic origin.

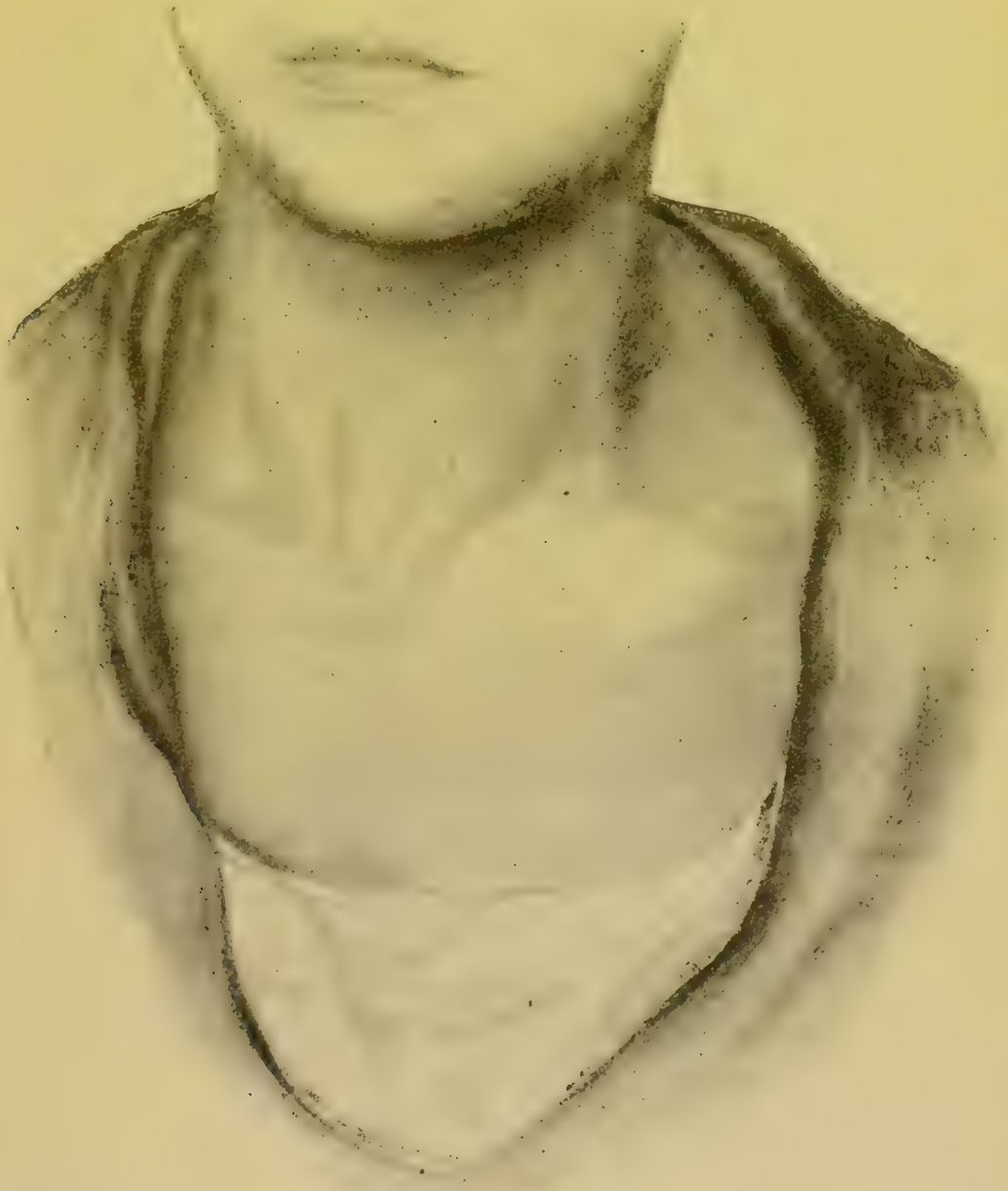
A fifth feature, peculiar to this group of maladies, is to be found in the fact that very often there are definite signs of implication of some other part of the nervous system, or of some special function. Thus, herpes is attended by very severe and peculiar pain, and when it ceases there is a numbness and defect of sensation in the tract of the part which has been affected.

Another point is that in nervous affections of the skin, although they are very frequently restricted to the skin itself, yet we must expect every now and then to find the muscles affected. It is known very well that this occurs occasionally in herpes zoster and it occurs yet more frequently in morphœa.

Lastly, a very remarkable feature in neurotic affections is that they are self-terminable. Not only do they not affect adjacent parts, but they tend, after going through certain stages, to spontaneous recovery. The restoration may never be complete if the

neurotic process has been severe, but there is always some attempt at it. Thus in herpes, in which the stages are short, the restoration is usually in the end complete, with the exception of scars, although there may have been for a long time great defect in sensation. The muscular paralyses of herpes are, I believe, always speedily recovered from. In morphœa, of which the stages are very prolonged, the recovery in young persons especially is never anything like complete. The illustrations given last week showed the peculiar appearances of shrinkage of one half of the face, usually known as hemiatrophy. This is not due to any advance of the disease, nor does it constitute any exception to what has been said as to the law of spontaneous tendency to recovery. The so-called hemiatrophy is simply a result of arrested growth, consequent on a disease long persistent in early life. So far from its being attended with any progressive increase, as some expressions of authors would suggest, it always becomes less and less conspicuous as the patient advances in life. In treating of neurotic diseases of the skin we must never forget that there are anatomical and physiological possibilities, allowing of very considerable variety both as regards the process and its result. Thus, the skin itself, so to speak, is an extended end-organ, or rather a collection of associated end-organs. Disease may take its beginning in these structures, just as in the case of the eye it may begin in the retina itself. Next, it is to be remembered that the skin comprises not only a complex arrangement of organs of sense, but also one almost equally complicated which regulates the supply of blood. Its vasomotor system may be diseased either separately or in combination with its sensory structures. Next we have the conducting fibres, and these of both kinds, and after these the ganglia, coming at last to the central structures in the cord and brain themselves. For the most part, in dealing with neurotic affections of the skin we are concerned with one or other of the varieties of what may be called peripheral neuritis. There may, however, be some which begin in the centres, and there may be others which beginning in the periphery, advance later on towards these parts. The gist of my argument is, then, that we must allow for great complexity, and

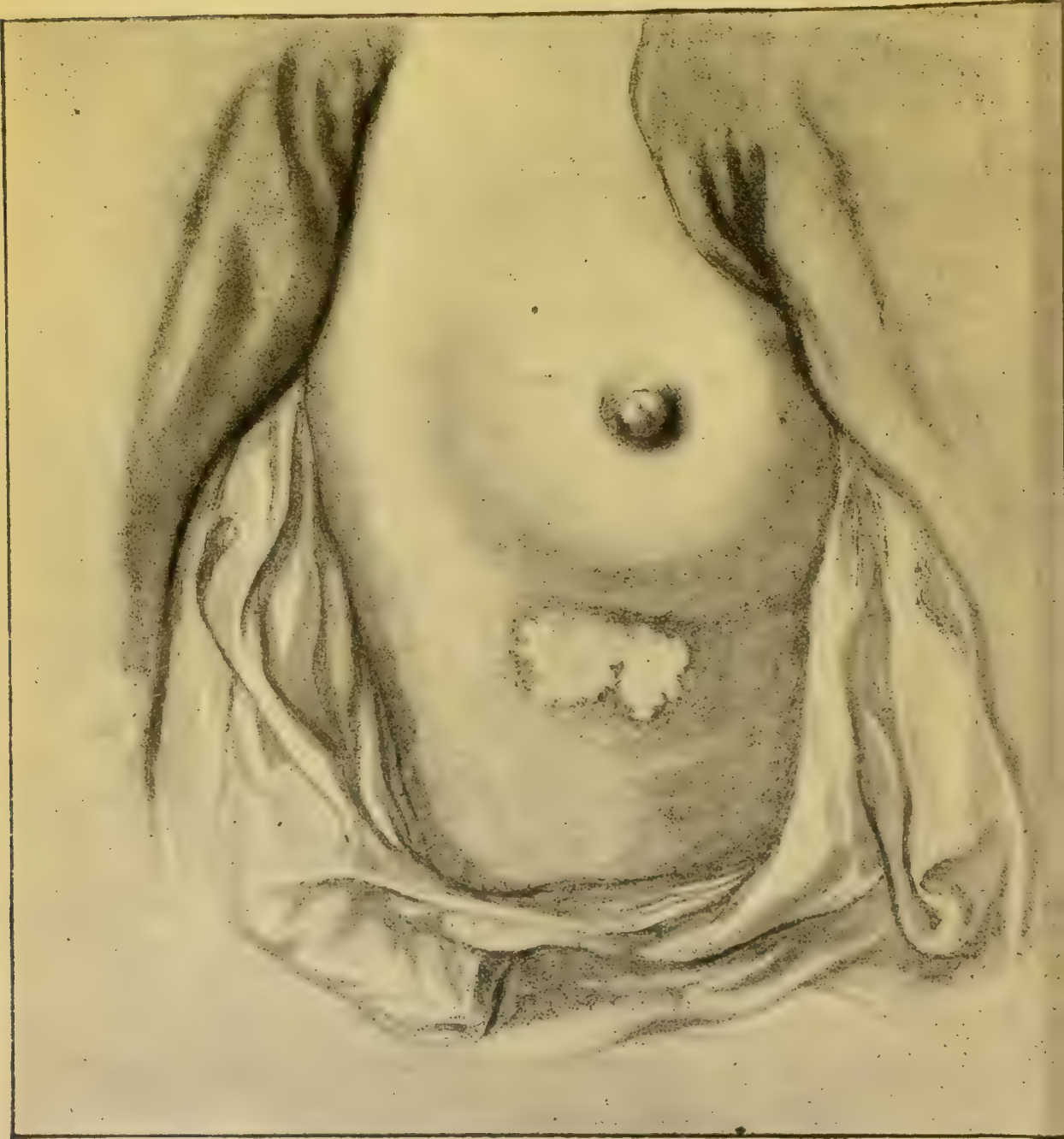
for many modifications of the manner in which neurotic affections of the skin develop; nor must it be forgotten that



Morpheæ, showing the Grouping of Spots.

processes which are functional only in the beginning may frequently, by repeated recurrence, tend to produce organic changes.

In corroboration of what I said in the address, as to localised morphœa not occurring in areate or circumscribed patches, I now



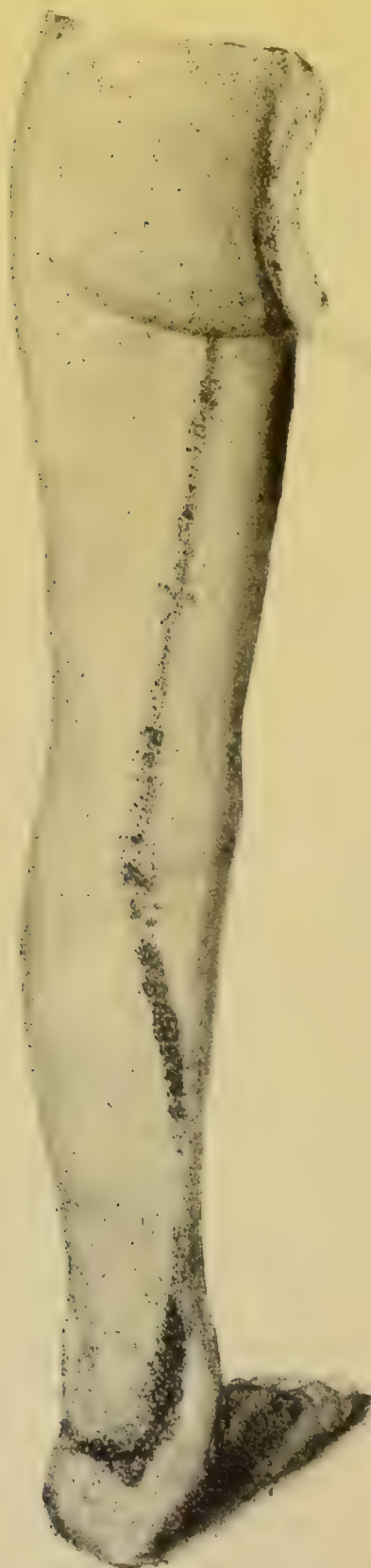
Patch of Morphœa, showing its Irregular Outline.

give two woodcuts, carefully copied from drawings in my possession. One of these shows a morphœa patch, of a long oval shape,

placed over the sternal half of the clavicle. It will be seen that the patch consists of a number of oval spots, which are thickest and most conspicuous in the centre, and arranged much like herpes. Had the morbid processes been of greater severity, or had they been observed even at a later stage of the same case, coalescence of the whole would have produced a single patch, and the term circumscribed might then have seemed appropriate. In the second woodcut the patch is shown just under the breast, at a later stage, with much larger amount of infiltration. It is the characteristic "ivory patch" of morphœa. It will be seen, however, that the patch is divided into two, and that its outline is neither a round nor an oval, but irregularly sinuous. Its little promontories have no doubt been produced by the coalescence of outlying spots with the chief or central group. I quite admit that there are many cases in which the ivory patch of morphœa presents a more rounded outline than is here shown, but must repeat my conviction that, even in these, careful observation of the various stages will lead to the conviction that the patch was constituted by the coalescence of many foci which were originally separated. Sometimes this is quite as definitely proved during the retrogression of the disease as in others during its onset. Thus, in a case which I have seen to-day, through the kindness of Dr. Herbert Miller, of Stoke Newington, a patch which two years ago was tolerably uniform and of considerable thickness has now broken up into several.

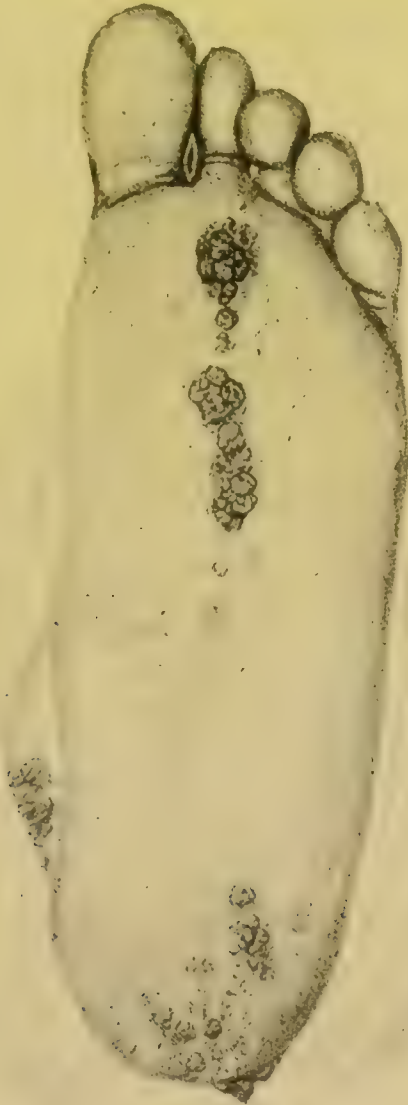
As regards the association of other nerve phenomena with morphœa, I may mention that I once examined a case in which the whole of one half of the face was affected, and the pupil of the eye on the same side was widely dilated, and its accommodation lost. For the opportunity of examining this case I was indebted to my friend Mr. Nettleship.

In the lecture as published last week a very remarkable illustration of unilateral white streaks occurring on the trunk and limbs of a Hindoo was given. For the portraits of this patient I was indebted many years ago to my friend Mr. Frederick Mackenzie, who had then recently returned from India. In this instance there was no tendency to ichthyotic developments on the



Ichthyotic Streak down one Leg (not over any known nerve).

streaks, there being simply loss of colour ; as the changes occurred, however, on a dark skin, the contrast was very marked. My object in reproducing this portrait was to show that whilst these congenital streaks are in the most definite manner unilateral, it was yet exceedingly difficult to assign them to any definite nerve dis-



Ichthyotic Streak on Sole of Foot.

tribution. In the delivery of my address I produced a considerable number of other portraits, taken from English people and from ichthyotic patients, but all of them illustrating exactly similar facts as regard local distribution. Amongst them was a

series of beautiful photographs given to me by Dr. Savill, of the Paddington Infirmary. From another case, one of my own patients, I have had copied this week the definite illustration which is here produced. A long, narrow, ichthyotic streak, not broader than the little finger, passes down the whole length of the back of the lower limb, in a direction which it is quite impossible to assign as the distribution of any nerve. From the same case is copied a second illustration, now given, showing a streak crossing the sole of the foot, and to it similar remarks are applicable.



*Sarcoma of the testicle, following on removal of the other
for hypertrophy of a doubtful nature, which the microscope
had declared not sarcomatous.*

By JONATHAN HUTCHINSON, F.R.S.

AN elderly gentleman, a member of our own profession, called on me with the express request that I would remove one of his testicles. He had already consulted Sir James Paget, and had been told that the conditions were very suspicious as regards malignant disease in an early stage. He was in good health, but was very anxious and unhappy from his fear of cancer. He was fond of hunting and thought that he might have bruised the testis on the saddle; there was no history, however, of any definite injury. The testicle was enlarged to about the size of a hen's egg and felt heavy and firm. It was the body of the gland which was involved, and the enlargement had been noticed for about six months and was steadily increasing. It was impossible to feel any certainty in the diagnosis, but the conditions were sufficiently suspicious to make me readily accede to the patient's wish. The testis was accordingly removed. Mr. Harrison Cripps, Mr. R. W. Parker, and others were present at the operation. On examining the testis by section after its removal, everyone thought that we had made a mistake in diagnosis, for it showed no appearances of new growth whatever. It was simply hardened and apparently hypertrophied. All parts in the cut surface looked much alike. The lobules and tubes were seen with remarkable distinctness, and on a larger scale than usual. We all of us took portions away for microscopical examination, and all my friends assured me afterwards that they could find nothing but the evidences of chronic inflammatory induration. Such being the case I told my patient that I did not think he need fear in the least any recurrence, and regarded the case as one in which an operation had been done without adequate cause. I am very sorry, as the sequel proved, that I did not think it worth while to preserve the specimen or to make a drawing of it, nor did I keep any microscopic sections, and I am indebted to the kindness of Mr. Harrison Cripps for the one which I produced at the Society. I may repeat, however, that

the testimony of all who examined it confirmed our naked-eye impressions that it was an example of inflammatory hypertrophy and not of new growth. About eighteen months, however, after the operation, Dr. H— returned to me with the statement that his remaining testis was beginning to enlarge just as the other had done. He had in the interval enjoyed good health, and his age was now nearly seventy. I found, as he said, the second testis presenting precisely the same conditions as the first had done. It was somewhat enlarged in all directions and felt dense and heavy. Remembering the negative results of examination on a former occasion, I was unwilling to do another operation and advised delay. When, however, about two months later he returned to me with very considerable increase, I took alarm and acceded to his wish. The second operation was performed on June 14th, 1883, the testis being at this time as large as a small fist and somewhat nodulated on its exterior. I possess portraits of the testis after removal, exhibiting its exterior and its appearances on section. The latter exhibits a greyish brown surface obscurely divided into lobes. It was moderately firm in structure, and did not yield much juice. It proved to be a good example of round-celled sarcoma. Dr. H— did well after the operation, and again recovered excellent health. Two years later, however, he slipped n descending some steps, and soon after this a growth appeared in the head of his tibia; probably it had been there before. After this, spontaneous fracture of the tibia occurred, and still later growths developed in the subcutaneous cellular tissue of several parts. He died early in January, 1886, two years and a half after the second operation, and about six after the first.

Several different opinions may be held respecting the facts which I have narrated. It may, in the first place, be held probable that the testis first affected was really not the seat of any morbid growth, but of inflammatory hypertrophy only, and that it had no direct connection with what subsequently happened. This seems to me improbable, for such enlargements are rare as the result of inflammation only, and the cause of the malady in the second testis was exactly like that of the first. Next, it may be supposed that a condition not to be distinguished from inflammatory hypertrophy was really introductory, and constituted indeed the first stage of sarcomatous growth. This is the view which I am myself most inclined to take, for it is in harmony with much

that we observe as regards the early development of external cancer. In epithelial cancer, as we see it on the lip, tongue, &c., there are very frequently, indeed I think usually, prolonged pre-cancerous periods, during which the local changes are those of chronic induration only. What we here see demonstrated beyond possibility of doubt in parts which are visible may very probably have its parallel in the viscera. Many forms of internal cancer may, it is not unlikely, have their introductory stage of chronic inflammation. In the case of the lymphatic glands in particular, everyone will allow that it is almost impossible to decide where inflammatory hypertrophy ends, and sarcomatous or lympho-sarcomatous processes begin. There is, however, yet another suggestion as explanatory of the facts of my case, and one probably which the majority of authorities would prefer to accept. It may have been that the disease was from its very beginning an infiltrating sarcoma and had nothing to do with inflammatory processes, and that it illustrates only the difficulty of distinguishing by the microscope as well as the unaided eye between the two. Very fortunately sections from the testis first removed had been preserved by Mr. Harrison Cripps, and concerning these I believe that the opinions of several who have recently seen them are in favour of their being examples of sarcoma. Others, however, declare that they show nothing that would justify any definite opinion, and I must claim to be entitled to allege that no one suggested they indicated sarcoma before the sequel of the case was known. I will not enter into any detail as regards the microscopic appearances, for I did not myself in the first instance study them carefully, having taken for granted from the naked-eye appearances that there was no new growth, and having been told also by my histological friends that such was their conclusion. It may, I suppose, be freely granted that the microscopic diagnosis between infiltrating round-celled sarcoma and chronic inflammatory hypertrophy is not always easy.

For clinical purposes the recognition of the early stage of malignant disease of the testicle is at the same time very important, and at the same time very difficult. If the testis continues to grow in spite of treatment, then the disease declares its nature. But should the patient come under care in an early period of the enlargement, it may for a time be almost impossible to give any definite opinion. I have had recently under care a case in which

the lesson taught in the narrative above given was repeatedly recalled to my mind. The patient was an elderly gentleman, who was brought to me in the belief that the disease was of the nature of new growth. The testis was hard and heavy, just as in Dr. H—'s case. There was the history of cancer in his family, and I had indeed myself amputated the lower limb of one of his sons on account of myeloid sarcoma. There was the history, however, of prostatic irritation having preceded the swelling of the testis, and this induced me to recommend that the operation which had been proposed should be deferred. After a few weeks of absolute rest in bed with iodide of potassium and the local use of cold the induration of the testis disappeared completely.

February 19th, 1889.

Report of the Morbid Growths Committee on Mr. Hutchinson's case of disease of the testicle.—We have examined microscopic sections of the testicle removed by Mr. Hutchinson on April 24th, 1880. One of the sections was prepared by Mr. Harrison Cripps, others by Mr. R. W. Parker.

Of the other testicle subsequently removed, neither microscopic sections nor the specimen are extant. We have no hesitation in regarding the disease as lympho-sarcoma. The structure of the new tissue is quite uniform, and is of the most typical lymphoid kind. The reticulum between the individual cells of the growth is beautifully seen in Mr. Cripps's preparation. There is nowhere the least indication of any transformation of the new substance into fibrous tissue, the cells of which substance are uniform in size and shape, and exhibit no enlargement, elongation, or other fibroblastic changes.

April 2nd, 1889.

R. W. PARKER.

SAMUEL G. SHATTOCK.

25

A Surgeon's Notes on certain Diseases and Malformations of the Teeth.

BY

JONATHAN HUTCHINSON, F.R.S., LL.D.,

Consulting Surgeon to the London Hospital.

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*A Surgeon's Notes on certain Diseases and
Malformations of the Teeth.*

BY JONATHAN HUTCHINSON, F.R.S., LL.D.,
Consulting Surgeon to the London Hospital.

My first acquaintance with the disease which is, I believe, known in the dental profession as “Riggs’ disease,” but for which I would propose the name *Sycosis dentium*, began with the observation of patients who had lost all their teeth before arriving at middle life, and in whom the usual history of dental caries, &c., with pain, was absent. The subjects of this condition had of course come under my observation quite accidentally in connection with other surgical maladies. But my interest was unavoidably excited by the fact that it was possible for a man, whilst apparently in good health, to lose all his teeth without obvious cause. At length it happened that a patient came to me in whom the condition was in progress. A good many of his teeth had already fallen, and others were becoming loose; there was no caries and no toothache, nor was there much of the general inflammation of the mouth which attends mercurial salivation. The gums were, however, somewhat

inflamed, but the inflammation was chiefly limited to the immediate neighbourhood of the tooth fangs. Around several of these, by a little pressure, pus might be made to exude. The disease was clearly, in the main, a suppurative inflammation of the tooth socket. Its similarity to the disease of the hair follicles which we know under the names of Sycosis and Tinea tarsi at once struck me. At this time I had never heard of Mr. Riggs' observations, nor was it until some time afterwards that I became aware that the condition had been observed by him and had received his name. My information upon this point came through a patient. A fellow-countryman of Mr. Riggs, an American, consulted me for some ailment, I forget now what, and I had occasion to examine his mouth. He observed the interest which I took in the loss of teeth from which he was suffering, and observed that he was the subject of "Riggs' disease." I took his diagnosis and at once made inquiries amongst my dental friends, and collected what information I could respecting it. To you, Mr. President, I was specially indebted for detailed information, and you told me when I mentioned my sycosis comparison that you had yourself compared the disease to baldness. The information which I have collected amounts in the main to this: that all dental observers agree as to the fact that there is a liability in some persons to have

the teeth fall out one after another without any material aching or pain, and with suppuration about the tooth socket. Further, I think it is agreed that no cause has as yet been assigned for this ailment, that it is not connected with syphilis, or with any special form of debility, but may occur to healthy adults, and is, perhaps, especially frequent in men. The disease does not commence by an attack of general stomatitis, but begins by discharge around the roots of one or two teeth, and then gradually spreads to others. It is very intractable under treatment, as indeed might be expected from our ignorance as to its cause. It is of slow progress, but may in persistent cases, and after many years, leave the patient quite edentulous.

Let us now contrast these facts with those which we know concerning ophthalmia tarsi. I take ophthalmia tarsi in preference to sycosis of the beard, or whisker, because there are different forms of the latter which pass under the same name. Some of these are cryptogamic. Permit me, however, to express a definite opinion that the common forms of sycosis of the beard, &c., are exactly the same as ophthalmia tarsi, and that both are inflammatory and not cryptogamic in their origin. Ophthalmia tarsi is a suppurative inflammation of the follicles of the eyelashes, which produces a contagious secretion, capable

of infecting other follicles, and thus causing the disease to spread. It occurs in young and rather delicate persons by preference, but may be sometimes seen in adults. Its connection with debility, or rather with want of tone, is, I think, very definite, but at the same time it must be admitted that it is often seen in those who are not debilitated in any special degree, and who consider themselves in excellent health. Sometimes the disease spreads to the eyebrows. If the patient be an adult male it frequently affects the whiskers, moustache, and beard; in rare instances it may attack the scalp, and yet more rarely it may affect the pubes and axillæ. I have known a patient rendered almost entirely bald by a pustular inflammation of the scalp affecting the hair follicles, and in almost all respects similar to ophthalmia tarsi, the latter malady being also present at the same time.

There is yet another malady which I think we must associate with the inflammatory and non-parasitic form of sycosis; it is one to which I ventured many years ago to give the name of sycosis of the nails (*sycosis unguium*). In this disease, which is rare, all the nails of both hands and feet are liable to suffer from suppurative inflammation at their roots. The nail becomes loosened, and may fall. The disease, so far as I have seen it, occurs only in cachectic and under-

fed children. It is clearly contagious, beginning on one digit and then spreading to others.

The methods of treatment which succeed in the cure of inflammatory sycosis, whether of the eyelids, scalp, face, or nails, is strongly in confirmation of the theory of contagion, and at the same time of the belief that there exists a constitutional debility, which is to some extent a predisposing cause. That the share taken by the constitutional predisposition is small, is proved by the fact that when once cured, as it may easily be by proper treatment, the disease does not as a rule show any tendency to return. I have seen this over and over again in very bad forms of ophthalmia tarsi (*sycosis tarsi*), and in some in which the whiskers and beard were affected. In the latter, however, the process of cure is so very different that we have much fewer opportunities of testing its permanency. The method of cure consists in acting vigorously up to the theory of its being a contagious inflammation. One or other of the remedies which repress cell growth when over-active must be employed; and of these by far the best is mercury. Great care must be taken to allow this agent to come in contact with the cell structures which are involved. To this end all crusts must be removed, and the eyelashes pulled out. If necessary, the epilation must be carefully repeated, and in its intervals a weak mercurial

ointment must be used. At the same time tonics must be employed. Under these measures it is very seldom that a complete cure cannot be obtained. If it is not, it is certainly due to the fact that the patient is out of health, and cannot adopt the measures for recuperation.

I shall have to point out directly how these facts may be applied to the treatment of sycosis of the teeth. Before doing so I may, however, mention a very remarkable instance which impressed me with the necessity for constitutional treatment. A gentleman of middle age, who simultaneously with one of his children suffered severely from sycosis tarsi, had for a considerable time resisted the influence of local treatment. He objected to epilation, but he had used the ointments perseveringly, and he did not get much better. He was not definitely out of health, but he was living in a town, and somewhat run down by work. I advised him to try the addition of champagne in addition to his tonics, knowing that this wine has in many cases a really marvellous influence over cell nutrition. He did so, continuing meanwhile the use of the ointment, and in a few weeks his eyelids were perfectly well. It is now six or eight years since the cure and he has never had any relapse. As regards the permanently toothless condition which results in the worst cases of Riggs' disease, we find its

parallel in certain forms of lippitudo, with entire destruction of the eyelashes as a consequence of sycosis tarsi. The fact, however, that hairs may grow again after exfoliation from disease, or after mechanical removal for purposes of treatment, and that this may occur over and over again, will explain the fact that their permanent and entire loss is a less frequent result of the disease than in the case of the teeth. It will also explain the far higher degree of intractability of treatment under treatment of Riggs' disease, for we cannot pull out the teeth in order to gain access to the disease underneath, as we do so freely in cases of sycosis of the hairs and nails. Although the comparison between hairs, nails, and teeth as to their development is in many respects very close, it differs in this, that there are no structures capable of reproducing teeth.

The next subject on which I propose to say a few words is one on which I must speak with great caution before the members of the Odontological Society. It is as to the occasional injurious influence upon the tongue and mouth generally of materials used for stopping the teeth. I have long entertained an opinion that some of the amalgams do occasionally cause sores on the tongue and mucous membrane of the lips, and I have expressed that opinion some years ago in public. I have little more to do on the present occasion than to

state that the experience of recent years has strengthened those impressions. I still very frequently see cases in which irritable sores on the tongue occur under conditions which oblige me to believe that the stopping used for the teeth is their cause. This never occurs in connection with stopping with gold or with gutta percha, but the material is always some black compound. So strongly have I been impressed with this fact that I always warn patients who are liable to sore tongue not to allow any black amalgam to be put into their teeth, and very frequently in intractible cases of sore mouth advise them to have all teeth which are so stopped to be either refilled with some other material or extracted. My statements on this point must be regarded simply as those of an observer. I am not prepared to enter into any detailed discussion on the chemical points involved, nor am I indeed acquainted in detail with the differences in the composition of the materials employed by the dental profession. Some of these are, I believe, secret preparations so far as the exact proportions used by manufacturers are concerned; and it may be that there are important differences amongst them as regards the possibility of their undergoing some sort of solution in the mouth. My dental friends assure me that these amalgams are practically indestructible, and that nothing is more uncommon than for these plugs to

exhibit any evidence of erosion or solution. Some of them admit, however, that inferior amalgams may possibly be dissolved. In particular I am told that there is a copper amalgam which stains the adjacent surface of the tooth, and which, therefore, to some extent presumably undergoes solution. However this may be, the fact remains that it is in my experience very usual to find a black stopped tooth opposite to an intractable sore on the tongue or lip, and to see the sore get well after the removal of the stopping or the tooth. Nor have the cases which have come under my observation been in patients who have been treated by incompetent practitioners, or with cheap materials.

One of the first cases which drew my attention to this subject still remains one of the most definite and instructive. It was that of a physician from the United States, who had a number of very intractable ulcers inside his lips and on the sides of his tongue. Everybody whom he consulted assured him they were syphilitic, but said he, "I am quite certain that I have never had syphilis." He was married and had healthy children, and he did not smoke. His teeth had suffered much from caries, and a great number of them were stopped with amalgam. The stopping had been done in America, and apparently well done. After much unsuccessful treatment by other means, I advised

him to get rid of all the stopping. He did so, and his mouth got well.

I do not intend to trouble the Society with any detailed remarks on the subject of *Syphilitic Teeth*. I have in fact nothing that is really new to say about it. It may, however, be not without its use to just briefly refer to the present state of opinion respecting them. I believe it may now be said that in all parts of the world the value of notched upper incisors is fully recognized as an indication of the previous occurrence of an attack of inherited syphilis in infancy.

I think it may be added that as time has gone on we have learned to concentrate our attention on these upper central incisors of the permanent set as being indeed "the test teeth." For myself I may say that year by year I find that I pay less and less attention to the peculiarities presented by the other teeth. Those peculiarities are often much more conspicuous than are the notches that are in the test teeth, but they are far less trustworthy. They are in fact misleading, and in a majority of cases are caused rather by the remedy which was given for the syphilis than by the syphilis itself. Respecting the test teeth, an experience of now upwards of thirty years enables me to speak with the greatest confidence. I do not recollect a single instance in which their malformation, if well characterized, has misled me.

When they are present, their evidence may, I believe, be trusted to the full. It may be well, however, here to say a few words respecting the numerous cases in which, in spite of the existence of inherited taint, the characteristic teeth are not present. I use the word characteristic advisedly, and in its strongest sense; for I suspect that there are not a great many cases in which these teeth present no peculiarity whatever. There are many in which their malformations are only slight, and such as cannot in any way be trusted as symptoms. The best opportunities which we get for observing the range within which syphilitic malformations of the teeth may vary, occur in cases in which several members of the same family have suffered from inherited taint. In such cases I have not infrequently known one or two of the children to present the characteristic teeth, whilst another in whom it was certain that the taint still existed showed little or no peculiarity. I may here note that the experience of recent years has confirmed, without however in any way explaining it, two little items of clinical observation which I made long ago. The first of these is that those subjects of inherited taint who present us with good examples of interstitial keratitis have almost invariably malformed teeth, and those who have malformed teeth scarcely ever escape interstitial keratitis. The other point is that those who are

liable to suffer in after life from phagadænic affections of the mouth or throat very often—indeed, perhaps, usually—show nothing peculiar in their teeth. Several remarkable illustrations of this latter statement have been quite recently under my observation.

If I turn now to the subject of stomatitic, or mercurial teeth, I am quite aware that I am entering upon a subject concerning which the propositions I ventured to make some years ago have not by any means met with general acceptance. Those propositions were to the effect that, in cases in which great and general damage to the enamel of the permanent set of teeth is observed, it ought to lead us to suspect that the patient has in early infancy passed through an attack of stomatitis, attended with inflammation of the tooth sacs. Subsequent experience has confirmed me in this opinion, and I feel more and more confident, as facts have increased in number, that by far the most important cause of defects in enamel development in the permanent set of teeth is the use during infancy of mercury in one or other form. I by no means assert, and never did assert, that mercury is the only cause of the stomatitis which produces what I ventured to call stomatitic teeth, but that it is by far the principal one. A knowledge of this fact has led me of late years to be far more careful than formerly as to

the use of mercury in infancy. The test teeth for mercury are the first molars of the second set. Their condition should be contrasted with that of the two premolars. If all the eight latter have good white enamel while all the four former show absence of enamel and spinous discoloured surfaces, then I believe we may with confidence assume that the child took mercury in infancy, or that some cause for a similar kind of stomatitis then existed. Of the concurrence of these teeth with lamellar cataract, a fact to which attention was drawn many years ago, I have had during the last few years some very remarkable cases in proof. Whilst admitting that it is to some extent still an open question whether there is any correlation of development between the eyes and teeth, I still hold to the belief that the connection between the two phenomena referred to is not to be so explained, but that the lamellar cataracts are due to convulsions in infancy, and the damage to the enamel of the teeth is due to stomatitis caused by mercury given for the convulsions.

Of late years I have been in the habit of using what I have called "mercurial teeth" as an assistance in reference to treatment in adult life. If a patient presents himself, we will say for syphilis, whose teeth show the mercurial malformations, I observe the greatest possible care in the administration of the drug in question. My attention was

first drawn to this point by the occurrence of very profuse ptyalism from very small doses of mercury, in a patient in whom I noticed afterwards that his teeth were most definitely of the mercurial stamp. It occurred to me at once as possible that he had an idiosyncrasy involving unusual susceptibility to the drug, and that it was owing to this idiosyncrasy that his teeth had suffered from what was given in infancy. If this view be correct, mercurial teeth may rank as revealing symptoms, and may be of value as giving us information as to individual susceptibilities which we could not have otherwise recognized.

The son of my friend Dr. R. offers a very good instance of mercurial teeth in association with lamellar cataract, and as his father is a medical man we have exceptionally good opportunity of obtaining the facts as to his infancy. The condition of lamellar cataract in both eyes is very marked, the opacity being very dense and the obstruction to vision such as to make operation desirable. It has been decided to use the needle. He is a clever sharp lad, and does not suffer much from headache or other inconveniences. The history is that in infancy he had laryngismus stridulus, and also a long succession of severe convulsions, and that for both of these affections he took much mercury. The result has been that his four first molars all show their crowns denuded of enamel and black

from discoloration; whilst his eight bicuspid are all perfectly formed and well covered with white enamel. His incisors and canines all show the enamel more or less defective, and their surfaces discoloured. At first sight I thought that there was an exception to the rule, and that one of the bicuspid was carious, but on more careful inspection I found that the carious tooth was really a temporary molar, and that a well-capped bicuspid was just coming up by its side.

The whole subject of the *correlation of the defects of the teeth with those of other organs* and parts is one worthy of careful study, and it is one with regard to which as yet but little has been done. My friend Mr. Moon has published in the Transactions of this Society two series of cases in which rat-like teeth—that is, teeth pointed by suppression of the lateral denticles—were present in several members of the same family, and in association with other defects. These defects were microphthalmos and deficient formation of hair. Mr. Moon's observations concerned the second set of teeth.

I have myself published in the Medico-Chirurgical Transactions, some years ago, a very remarkable case in which a male child was born almost hairless, and with great defect in the development of his skin. In particular his mammary glands were absent. There was no obvious

defect in his eyes, and his teeth, which were his first set, were only peculiar in being small and not placed straight. He had no microphthalmos. We may take his case in proof that extreme defects in the formation of the skin and its appendages are not necessarily attended by defects in the development of the teeth. This fact has been further illustrated by one observed by my son, Procter S. Hutchinson, in which a puppy born almost hairless showed no defect in its milk teeth. It would be of much interest in connection with this subject, if surgeons engaged in ophthalmic practice, and others who may have opportunities, would in all cases of microphthalmos make careful note as to the state of the teeth. We want particulars in these cases both of the first and the second sets. Conversely, if those engaged in dental practice would in all cases of "rat-teeth," or other remarkable defects of development, keep note of the state of the eyes, hair, and nails, we should get a class of facts which might help us to avoid error, and possibly give an insight into new laws as to correlation.

I have to repeat, what I have said formerly as to the dentition of those that are ricketty, I do not believe that the subjects of rickets, as a rule, show any definite peculiarities in the development of their permanent teeth. I have carefully availed myself of all opportunities for observation

and feel sure that the term "ricketty teeth" is used, when it is used, without any definite meaning.

It is perhaps time that something more should be attempted with a view to arrive at knowledge as to the significance of the tooth structure as an index of that of the other tissues of the body. If the teeth wear well or badly, are we justified in either case in assuming anything as to the longevity and capacity for wear of other tissues? In any investigations on these points we must carefully discriminate as to the different forms of tooth disease. It is not sufficient to state the mere loss of teeth, we must know from what cause they were lost. The facts as to Riggs' disease, if I have got a correct impression respecting them, show that the teeth may be lost without there being any inherent defect in their structure. In this affection the teeth, as yet sound, fall out from inflammation of their sockets. Such cases are wholly different from those in which the teeth suffer from premature caries, or, as the patients call it, "crumble away." In this latter group of cases it is very improbable that the teeth are the only tissues in the body which have been badly developed, and, did we fully understand the bearing of the condition, it might help us materially to a recognition of the patient's state of health and life prospects.

Not only ought we, I may suggest, to try to learn all we can as to the vital significance of early decay

of teeth, but we may perhaps make some use of their tendency or otherwise to accumulate tartar. This tendency unquestionably runs in families. I have observed it in a very marked degree in members of three generations, and have seen it under conditions which would imply that it had very little to do with neglect of attention to the toothbrush. Some persons indeed, who are able to prevent the collection of tartar on the outer surfaces, are quite unable to do so as regards the inner surfaces of the incisors. I have little doubt that excessive tendency to tartar is in many instances a proof of habitually excessive flow of saliva, and I believe further that it often occurs in those who have a tendency to gout.

It is not often that I trespass on the domain of the dental profession in respect to the scaling of teeth. I was, however, consulted the other day by a lady who believed she had a bone growing in her mouth, and for whom I had to remove, not without some force, the fragment which I now exhibit. I may own that at first glance I was puzzled as to what it was, for it was quite smooth and polished, and looked in the mouth twice the size that it does out. She was a maiden lady of most particular habits, and it will be seen that the accretion is grooved by the use of the toothbrush. There was gout in the family.

As regards *observations on the milk teeth*, I

suppose it may be taken for granted that they will usually display the results of disturbances of the health of the fœtus during intra-uterine life rather than of anything which has occurred after birth. Thus they usually escape malformation in inherited syphilis, though very liable to suffer from caries, and to fall out early. It would be of much interest if those engaged in family practice would, in all cases in which pregnant women suffer from severe illness, and more particularly when they have to take a course of mercury, note the state of the infant's first dentition.

I will now, with your permission, pass to another subject, and one which I do not feel quite sure that I am in good taste to mention at all in the present meeting.

I have recently seen two cases in which at the age of fifty the whole set of thirty-two teeth were in perfect condition. This is no doubt very exceptional, and it may be worth a few lines to record the conditions as regards inheritance, &c., which attended it.

In one case my patient was a single man born in Cornwall, but bred up from childhood in a wild district of the United States, where he suffered from "dumb ague." For this he took calomel and quinine, and of recent years he has taken much mercury for syphilis. In spite of this, however,

he has never in his life had toothache, he has not a single spot of caries, and his teeth are all beautifully white and in perfect order. He is a man of very precise habits, and has for long been accustomed to clean the teeth three times a day, using a rather soft brush and some powdered chalk which he scrapes for himself. Once or twice a year he carefully scales the lower incisors behind free of tartar. He is a spare man, of sound health and temperate habits. His parents lived to be old, but his mother had been thought to be consumptive. He believes that his father had excellent teeth, but his mother not so; one or two of his brothers and sisters have very good teeth, but one brother has suffered much from toothache. There is no history of gout in the family, and no peculiarities as to diet are to be noted.

In my second case the gentleman was decidedly gouty, and I quoted to him the proverb, "You have got a fine set of teeth, mind you don't dig your grave with them." He was fifty-four years of age, and his set of teeth was absolutely perfect; none had been lost, and none showed any tendency to decay. With the exception that he had suffered several times from gout, and from gouty dyspepsia, he had enjoyed good health. I did not get any facts as to hereditary history. His teeth were clean and white, and had evidently been taken good care of.

The only explanation of the proverb I have mentioned that I know of is that those who have good teeth are prone to be large eaters, and may in this way be likely to develop indigestion, gout, and other maladies due to the over-supply of food. Some of my medical friends are, I know, in the habit of speaking of "gouty teeth." With the exception that I think we not very infrequently find, in association with the general vigour of constitution and soundness of tissues which members of gouty families often exhibit, a more than usually good preservation of the teeth, I do not know of any peculiarities which denote gout. Those who preserve their teeth well often show, simply as a consequence of their long use, a remarkable degree of wearing down. In some instances this wearing down may be due to the habit of grinding the teeth, and Dr. Graves has taught us that a remarkable tendency to teeth-grinding is usually indicative of gout. He records some remarkable instances in which gouty patients suffered extremely from tooth-grinding, the teeth being worn level with the gums, and the habit becoming so incessant that the patients were obliged to avoid all society. I am not aware that any of the more recent writers on gout have recorded any similar cases. No example of persistent tendency to tooth-grinding in a gouty patient has ever fallen under my notice.

The liability to the wearing of teeth by friction,

whether in mastication or otherwise, evidently differs very much in different persons. In the first of the two cases which I have just described, the man was in the habit of brushing his teeth three times a day, yet not the slightest evidence of wearing from the brush was present. I have seen mouths in which a furrow produced by the toothbrush crossed the necks of the incisor teeth so deeply as almost to cut them in two. The worst that I ever saw of this kind was in the person of a dentist, who assured me that such toothbrush furrows were not by any means uncommon. The pipe notch caused by smoking, although not unfrequently seen, is yet sufficiently rare to make it evident that its production depends to a large extent on some individual peculiarity in the construction of the teeth.



ON THE LAWS OF CONTAGION, FROM A CLINICAL STANDPOINT.

AN ADDRESS DELIVERED AT A MEETING OF THE MANCHESTER
MEDICAL SOCIETY.

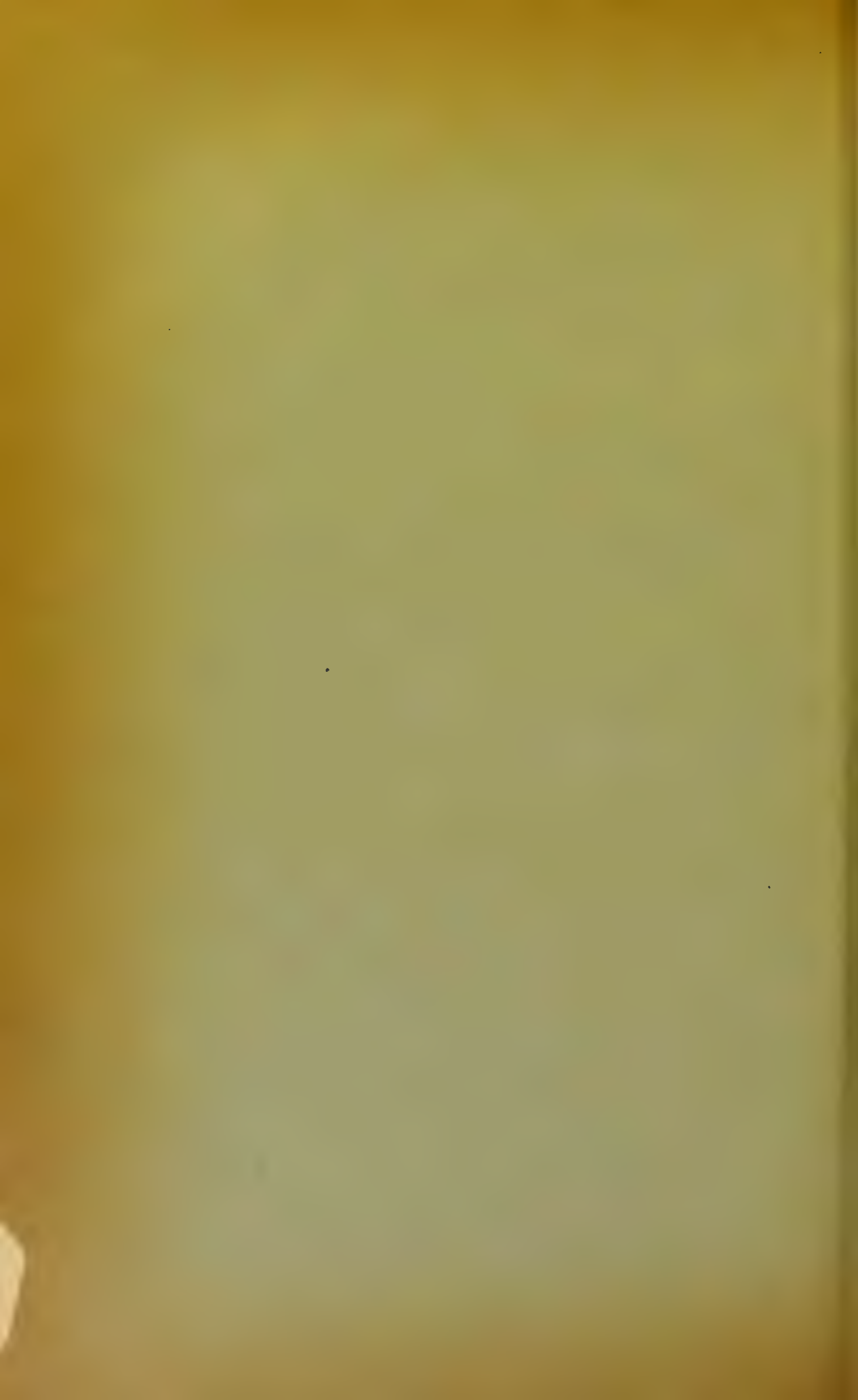
BY

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Late President of the Royal College of Surgeons.



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ON THE LAWS OF CONTAGION, FROM A CLINICAL STANDPOINT.

MR. PRESIDENT AND GENTLEMEN,—With your permission, I will begin by a general assertion that all the various forms of the inflammatory process are attended by the production of something which is capable of spreading that process by contagion. The only exceptions to this statement with which I am acquainted are those mysterious inflammations in which the peripheral nerves are concerned, and of which herpes and morphœa are the types, and probably almost the sole examples. In these, by a nearly invariable rule, nothing of the nature of infective spreading ever takes place. The extent of the disease is declared at the onset, and no advance is ever afterwards made. In all others, whether the local process be due to injury, to the presence of a cryptogam or a burrowing insect, or to an animal poison; whether the type assumed be erysipelatous or urticarial, eczematous or furunculoid, whether we call the resulting malady diphtheria, pleurisy, pneumonia, or carbuncle, there is risk of infective spreading. That risk is not always the same either in degree or in duration, but it is always there.

It is to the examination of the laws under which inflammation exercises its powers of infection that I have to ask your attention this evening. I will endeavour to be as concise as possible, and, in the attempt to attain that end, must ask you to pardon any expressions which may seem too dogmatic.

It is desirable perhaps to interpose a few definitions. The term infective must, I think, be used, as it now commonly is, in its widest sense, and not as being distinctive from contagion. Although in my student days I was taught that an infectious disease was one conveyed from person to person by the air, and a contagious disease one which required actual contact, yet I think that general usage has ignored such distinction, and employs the words much as if they had the same meaning. We must, therefore, when we intend to designate infection through the medium of the air employ some precise adjective, and speak of aerial-convection, air-convection, or air-infection. When a morbid

process tends to spread in the tissues of its original subject, we say that he is autoinfective, or self-infective, and with a little straining of language, pardonable under the circumstances, we apply the term to the disease as well as to the patient. If the evidences of self-infection occur in close proximity to the part first attacked we speak of such neighbouring results as satellites, and we imply by that term that the material of contagion has probably spread, not by the blood, but through the solid tissues, or by the lymphatic spaces. If, on the other hand, the disease has shown a tendency to become general, we think of blood-infection. Under such conditions we speak of diffuse or general reproduction, and we recognise that this may be either accurately symmetrical, or only partially so.

The terms specific and specialised must also be carefully used with distinct meanings. A specific disease is one which can be produced only by infection with germs which always produce the same results. A specialised disease is any malady which, without regard to unity of cause, and possibly as the result of a combination of several, has nevertheless acquired peculiarities and attained to individuality.

Before proceeding to speak of the laws of contagion, I will briefly enumerate the principal means by which, so far as is at present known, it may be effected:—

(1) By some insect, or other well-organised external parasite, which may breed on, or in, the tissues of the body, and transfer itself from part to part—*e.g.*, scabies.

(2) By some lower form of animal life of which psorosperms may serve as an example.

(3) By some low form of vegetable organism of which the moulds may stand as examples—*e.g.*, the *trichophyton* group of skin diseases.

(4) By some yet more lowly vegetable organism, capable of very rapid multiplication, and often in the blood as well as in the tissues, of which the various forms of bacillus are illustrations—*e.g.*, pyæmia, anthrax, gonorrhœa.

(5) By some, as yet, unknown form of virus (probably microphytic), to which the term “specific animal poison” is, in our present ignorance, applicable—*e.g.*, smallpox and the other exanthemata, syphilis, etc.

(6) By some inorganic substance of the nature of aptomaine.

(7) Lastly, it must be asserted to be possible that the cell structures of the body itself, independently of the introduction of any parasite or the development of any chemical poison, may assume qualities enabling them to convey disease to the parts with which they come in contact.

The list which I here submit to you has, of course, no claim to be more than a mere recapitulation for the guidance of the investigator. There is, obviously, no strong line of demarcation between several of the

groups. Concerning the last, especially, it is, perhaps, desirable to add a few words of explanation. The results of modern discovery have tended to greatly widen our conceptions as to the sphere of parasitic elements in the production of disease. We are now scarcely able to think of contagion without at the same time calling up the image of a bacillus as its cause. It may be that in this direction we are going too far and too fast. At any rate, it will, I think, be admitted that it is desirable for the present to keep in mind the possibility that the tissues of the body itself may, without any parasitic addition, become themselves the causes of infective spreading. This I would submit is a possibility under most of the various conditions of disturbed nutrition, whether we name them new growths or inflammations. The phenomena of infective spreading in the case of Melanosis are very instructive. They are, by way of example, definite beyond the possibility of misconception. From one little pigmented mass a thousand just like it are produced, and are scattered in every part of the body. It would appear to be an example of contagion by cells, or, at any rate, by cell-forming plasma. There is certainly no strong presumption that parasites have any share in it. What is here true of the infection-process is true also, with modifications, of all forms of malignant new growth, or, as we had perhaps better name them, malignant inflammations. Similar laws not improbably apply to other conditions, which, without any dispute, will be allowed to stand in the category of inflammations. I am thinking of boils, of acne, of contagious eczema, of psoriasis, lichen planus, and a multitude of other ailments in which the mode of spreading, multiplication, and also our methods of cure, favour the theory that they advance by the intervention of a contagious element. It may be that in many of these the contagious material is neither a bacillus, a psorosperm, nor a ptomaine, but a living cell, the product of the tissue whose nutrition has been disturbed. In connection with this important part of my subject I must ask attention to the important law that "when once a focus of local inflammation has developed for itself peculiar and, so to speak, individual characters, it tends in all its subsequent stages of infection-spreading to keep to its type." This is illustrated in all the diseases which I have just named, and in many others. In order to explain it, we must resort, I think, to one of two hypotheses—either the inflammation has been produced by some specific contagion, or it has, in the course of its evolution, produced something equivalent to such.

In order that I may illustrate what has just been said I will ask your attention to one of the very rarest of maladies; I allude to infective angioma of the skin. I show you two drawings, from different patients, which will, I think, convince all that this morbid process keeps to type. The two are exactly alike. I do not know of more than four examples of it

on record, and they were all exactly alike and quite different from everything else. The peculiarities of the malady are that it begins, in early life, by what looks like a very superficial port-wine-stain. This stain advances gradually and forms faintly marked rings which, in the course of years, cover the whole limb. They are attended by what may be called "cayenne-pepper grains," little tufts of dilated capillaries, in which blood is impounded and which you cannot empty by pressure. Our interest with the malady on the present occasion is its infectivity. About this there can be no doubt, for it spreads by continuity with its border and also by the production of satellites near to it. Clearly one explanation of its always assuming the same features is that it always affects the same structures. It keeps to the small blood vessels of the skin and travels in their coats. But what makes it travel? What is the cause of its infectivity? Is it more probable that it is some bacillus-parasite having a special fondness for these structures and for none others, or is it more probably a changed condition in the cell structures of the perivascular spaces, which is contagious from one to another? The great rarity of the disease, and the fact that it always begins in infancy, seem to favour the latter view. If it were due to a specific bacillus it is scarcely likely that we should have identified one case in Norwich, one in Canterbury, one in Berlin, and one in Edinburgh (as has been the case), and none elsewhere. I mention this disease the more willingly as an example, because it is so rare, but it is in all essentials like some others much more common. Whoever can explain for us the infectious spreading of serpiginous angioma will at the same time probably explain that of lupus erythmatus and many other peculiar varieties of the lupus family. These all spread serpiginously just in the same way.

If time permitted I would ask you to look at another series of drawings which I have with me illustrating another rare malady which needs an explanation of its contagiousness.

The disease to which the name of Rhinoscleroma has been given by Hebra may serve very conveniently to illustrate several of the points for which I have been contending. In it a chronic inflammatory process is attended by thickening and induration of the structures about the orifice of the nostrils. The disease runs a slow and very definite clinical course, and is attended usually after a duration of some years by similar disease in the palate and throat. Obviously it is an infective malady, and obviously it begins as an inflammation. Its usual exciting cause is, I believe, excoriation of the nostrils by catarrhal discharges. Recent investigators have identified in some cases a bacillus probably allied to that of tuberculosis and occasionally present in lupus. Yet the disease is in its phenomena very different from both lupus and tuberculosis. It is a very rare malady, and especially so in England and America. The

drawings which I show you are from Vienna, India, and England. I beg you to note their close similarity one to another, and I ask you, as in the instance of serpiginous angioma, is it a probable hypothesis that there exists a specific bacillus, the sole and efficient cause of this remarkable affection, which, although present all over the world, affects not one in a million of the population?

I will now pass to a different department of my subject and will endeavour to illustrate some of the laws of contagion by reference to common maladies.

We are not to think of the itch insect making its burrows in the skin simply as we should of a pholas boring into a harbour-pile; or, perhaps still better, a mole seeking his food in a meadow. We have to reckon rather with the results of irritation to a living structure, and the fact of life in the object attacked introduces at once important complications. The skin, irritated by the acarus, inflames, and the products of inflammation are in themselves infectious, and tend to spread disease. In many patients three-fourths of the eruption called scabies is caused by the contagion of inflammation-products, and not directly by the parasite. The eruption may indeed spread to parts at a considerable distance from the burrows of the acarus, and unless properly treated may persist long after all the acari have been killed. No two persons are alike in their proneness to develop these secondary inflammations. As a general rule, the younger the patient the greater the amount of inflammatory complication; but the difference is by no means one solely of age. Of two persons of the same age, and apparently in equally good health, one may show only the burrows and characteristic vesicles, with little or no surrounding inflammation, whilst another may be covered from neck to foot by an impetigo-eczema, the result of general infection by inflammatory products. The cure of many cases of scabies is that of eczematous dermatitis rather than the mere destruction of the parasite. Precisely similar statements are applicable to all the other irritative parasites, whether animal or vegetable. We all know how ringworm may pass into kerion, and how favus may sometimes entirely destroy the scalp, and even in rare cases affect the nutrition of the bones. There are other lessons in reference to the general doctrines of contagion well worthy the attention of the pathologist, which are to be learnt from observation of the diseases of the skin due to vegetable parasites. We are here dealing with simple and known causes, and from them we may argue as to the probabilities under more complex conditions. We may then profitably note respecting such a disease as tinea versicolor that its fungus grows with much greater freedom on the skins of some persons than others; that it may in one man persist for many years as a few spots only, whilst in another it will cover the whole trunk.

There is a curious group of diseases which are contagious, not only upon the patient himself, but also to others, which although not in the least resembling specific fevers, are yet subject to some law which brings them to an early termination. The rare malady known as *keratosis follicularis*, and that recently designated *pityriasis rosea*, are examples of this class. But the one best known, and which on that account I will take as an example, is *molluscum contagiosum*. This malady, not infrequent in young people, but occurring also occasionally in the middle-aged, and even in those advanced in life, is unattended by any derangement of the general health. It usually begins as a single spot or group of spots, around which others rapidly develop. It rarely becomes general, although it may cover a considerable extent of surface. It never lasts indefinitely, and I do not think that I have ever in my own experience seen a case which had a twelve months' history. As to its causes, excepting that it is probably invariably the result of contagion, and that in some obscure way it is, in adults, connected with the use of the Turkish bath, we know nothing. No one can doubt that it has some quite special cause, as special and specific, in all probability, as is that of scabies. Whether it is of the nature of a *psorosperm* must as yet be held to be doubtful, and it is in the present state of our knowledge quite impossible to give any explanation of the invariable fact of spontaneous cure. It is meanwhile of great importance to keep that fact in mind, since it is quite possible that some other maladies of a less definite nature come under the same law.

The Erysipelatous type of inflammation is one which offers, in reference to the doctrine of contagion, numerous points of great interest. No one doubts that erysipelas is self-infectious, *i.e.*, that it spreads locally on the skin of the patient. Most usually it travels by continuity of issue and without breaks, but not unfrequently satellite patches may be observed, and in rare instances, if I mistake not, something closely allied to symmetrical generalisation is witnessed. As regards its transference to other persons, no one, I suppose, doubts that this may occur whenever a breach of surface is exposed to contact with the contagious material. It is established beyond a doubt that erysipelas can spread in hospitals, and that it may affect not only the skin, but mucous and serous membranes, and that in this way erysipelatous dermatitis may be transmuted into puerperal metro-peritonitis. Our experimental investigators have even succeeded in isolating the parasitic organism which is supposed to be the cause of the spreading of erysipelas. Yet in connection with these established facts we may suitably call to mind that erysipelas is, after all, very rarely indeed prevalent as an epidemic, that medical men and nurses do not as a rule catch it from their patients, and that in private practice we seldom see more than one case in the

same house. It appears to be certain that the virulence of its contagious property is capable of great intensification, and that for the most part it is but feeble. The facts on record in reference to puerperal fever prove, however, that the virus may be conveyed by the hands or clothing of a medical attendant, himself in perfect health, during periods of many weeks together.

Having argued just now that the contagiousness of Erysipelas is an element which may vary very widely in its degree, I next have to ask your attentive consideration of the clinical facts which show that the disease itself does not keep closely to any one type. Nothing is easier than to foreclose such a discussion by the assertion that traumatic erysipelas is one thing and idiopathic erysipelas another, and that the one is probably due to bacillary infection and the other capable of spontaneous origin. Dogmatism of this kind cannot, however, settle such a question, and it will still remain for the truth-seekers in pathology to search out the facts which underlie the connection between the two. You will see that we are here face to face with an all-important question in reference to the doctrines of contagion, and that we have to ask whether it is possible for a disease which has originated independently of contagion, and in connection with some common cause of inflammatory action, to acquire for itself microphytic or other organisms, and to become virulently contagious. Are we to say of erysipelas that it stands side by side with smallpox and measles, as a wholly peculiar and specific malady, capable of production only by specific seed-sowing, or are we rather to consider it as a somewhat ill-defined form of inflammation of skin and mucous membranes, susceptible of variations within very wide limits and spreading only exceptionally from person to person by means of contagion? The first supposition would seem to me, however tempting from a histological standpoint, to ignore arbitrarily a whole army of facts. Let us turn for a few moments to the consideration of what is known as "medical erysipelas." First among the facts, as regards this malady, we recognise that those who have had one attack become liable to others, and that the disease, as a rule, diminishes in severity when it is frequently recurrent. The first attack of erysipelas of this type is almost always due to some local exposure, such, for instance, as to a cutting wind, a draught of air through an open window, or to the glare of sunlight reflected from snow. Cases which have originated from such causes may manifest precisely similar phenomena to those displayed by the traumatic form. The disease will spread over large tracts with a running edge. It is attended by vesication and much œdematous swelling; it is productive of high fever, and may be followed by death. That these cases of medical erysipelas are occasionally contagious to nurses, and that it is very dangerous to admit

them into surgical wards, are facts concerning which I have myself no doubt, and you will see that it follows from their admission that I believe that idiopathic and traumatic erysipelas are the same malady so far as the means of its spreading from patient to patient are concerned. Those, however, who accept this doctrine must see clearly what lies before them. The group of maladies closely cognate with idiopathic erysipelas is a very large and very interesting one, and does not keep at all closely to one type. In some cases, or, perhaps, it would be better to say in some persons, the inflammation may be evoked by causes wholly different from those which I have mentioned. Thus, we may have an arnica-erysipelas, a vaccination-erysipelas, and from whatever cause it have originated it will leave behind some tendency to its own reproduction. Everybody knows that patients who have had erysipelas once, whatever may have been its type or kind, are dangerous subjects for surgical operations, and are very prone to have the disease re-develop in connection with any wound. The disease known as elephantiasis, or elephantoid hypertrophy of limbs, almost always takes its origin in an attack of erysipelas, and runs its course by periodic recurrences of the same. The solid œdema of the face, which often produces such remarkable disfigurement, is again constantly attended by recurrences of erysipelas. There is a modified form of this latter malady in which the face becomes almost permanently eczematous, but with remarkable exacerbations, during which the eyelids swell up, and to which the name eczema-erysipelas is applicable. This disease, like most of the other peculiar forms, begins by an attack not distinguishable from idiopathic erysipelas, and assumes its own peculiar characters as it progresses.

I have been detailed in what I have ventured to say respecting the relations of erysipelas on the one hand to the ordinary causes of inflammatory action, and on the other to some more or less specific contagion. I have done so in the hope of throwing some light upon the problems to which I have now to ask your attention. Dermatologists in the metropolis have, as many will be aware, from what has been written in the journals, been much interested, during the summer which is just passed, with what has been called "an epidemic of eczema" in two of our largest work-house infirmaries. Dr. Savill, of the Paddington Infirmary, was, I believe, the first to call attention to a group of about 30 cases which were at the same time under his care in the infirmary of which he has the medical charge. He was followed immediately by a communication from Dr. Lunn, of the Marylebone Asylum, where precisely the same thing was taking place. Dr. Lunn has had, during the summer, not less than 100 cases under his charge. Through the courtesy of the two gentlemen I have named I have been permitted opportunities of becoming acquainted with the facts and of seeing the cases. I will briefly say to you now

that these eczema epidemics seem to me to prove, very conclusively, what some of us had long ago suspected, that eczema may, in certain forms, become contagious from person to person. That it is self-contagious—that is, that it spreads by its own infection on the skin of the sufferer, is a doctrine which I have long taught in the most explicit terms that I could find.

The facts as to contagion in the case of ordinary Boils must detain us for a moment. It is well known that although boils are often solitary, yet they also often come in crops, and that when once the liability to them has commenced, it is very apt to persist. I do not think that any one who has observed the phenomena of a crop of boils, or of persisting liability to them, can doubt that the element of contagion takes its share in their production. The ill-health which often attends them is more probably produced by them than in the relation of cause. It is usually not difficult, by the simple expedient of covering the part with a leather plaister on the very first day that the boil is threatened, to entirely prevent its development, and by preventing the one you prevent the whole crop. When a succession occurs, they are almost always produced in close proximity to the original one, being, as it were, satellites to it. The cure of a tendency to boils consists not so much in attention to the general health, though that may be important, but in the suppression, by local treatment, of the individual furuncles, and thus breaking the chain of contagion.

There are forms of pustular skin disease to which the terms Impetigo and Ecthyma are applicable, in which yet more definitely than in common boils the fact of contagious spreading is demonstrated. Large areas of skin may become covered with them, and no treatment other than local measures is of any material avail. The success of the latter, however, usually completely proves the local nature of the malady. These affections are to be placed in the same group with boils, differing chiefly from the latter in the small size of the furuncle which is developed.

We have yet another malady of the furuncular class, which obviously owes its character to its locally infective qualities, but which instead of producing satellites and a group of detached pustules, spreads from the original spot in depth and width. The form of dermatitis known as a Carbuncle begins as a boil, but manifesting very active qualities of self-infection, almost resembling those of erysipelas, it spreads widely in all directions. In the not unfrequent cases in which it ends fatally, death usually occurs before this process of infective spreading has come to an end. The object of treatment is, of course, to put an end to this process of circumferential spreading, and we attain that object by local and not by constitutional measures. Although this fact is to be distinctly asserted, and although we may venture to add to it with great confidence

the statement that in the majority of cases carbuncles occur, as a sort of accident, to persons in good health, and might have been arrested in the early stage by appropriate treatment, yet we must also admit that in many cases constitutional measures take an important share. The liability to carbuncles to develop extensively in those who are the subjects of diabetes is well known. The admission of this fact may perhaps afford me a suitable opportunity for discussing briefly the possible influence of blood-conditions upon the development of morbid processes which have taken their origin quite independently of them. There is nothing in the least inconsistent with the theory of the local and almost accidental origin of boils and carbuncles and the admission that they develop more rapidly and more widely when there is sugar present in the blood. Probably, indeed, we have here only an illustration of a law of wide significance. It may be that in all cases in which the infective spreading of an inflammatory process is observed, that process is greatly under the influence of the food which is supplied to the tissues. We can easily understand that such may be the case with the specific bacilli. It would appear that a free supply of the animal fats is unfavourable to the development of the bacillus of tuberculosis. It is quite possible, may I not say even probable, that the bacillus of leprosy is much assisted in its development by a liberal supply of salt fish. And respecting the bacillus of anthrax, a disease chiefly incident to ruminants, it is held by at least one competent observer that its growth in the human subject may be stopped by refusing a supply of vegetable food. "Upon what meat does this our Cæsar feed, that he is grown so great?" is a question which we may do wisely to ask ourselves respecting each and all of the parasitic organisms which cause disease. It is not merely the introduction of the parasite into the body, but its supply of suitable food, which influences the production of a specific disease. Obviously there is something in the presence of mercury in the blood which is prejudicial to the life of the virus of syphilis. It has been supposed that the use of iodide of potassium very much favours the extension of erysipelas, and it may easily be the fact that there is something in the diabetic blood which helps that of carbuncle.

Perhaps there is no disease in the treatment of which it is more necessary to recognise the fact of contagious spreading than in common Acne. Although it is, of course, beyond doubt that the production of acne has relation to the age of the patient and the thickness and coarseness of his skin, yet I hold that it is equally beyond doubt that when large crops of acne spots are present they are the consequences of contagion from their predecessors. Constitutional treatment avails little or nothing. You may give purgatives, tonics or arsenic, to any extent, but if you neglect the local treatment of acne it will persist. On the other hand, although by no means recommending the omission of

constitutional measures, I yet venture to assert that persistent attention to local treatment will cure even the worst cases. These measures must be much the same in character as those necessary for boils. Acne pustules are indeed to be regarded as being a modified form of furuncle. They are to be destroyed or rendered abortive by the application of some caustic, such as the acid nitrate of mercury, or by the use of some ointment or lotion containing that mineral. It may be of interest in connection with acne to make the observation that there is nothing in the fact that a disease occurs symmetrically and manifests a distinct preference for certain localities, which stamps it as definitely, or as by any means certainly, of constitutional origin. Undoubtedly, whenever such tendency is observed, constitutional, or in many cases, anatomical, explanations are to be sought for it. But there is a partnership both as regards local and general causation, and neither anatomical peculiarity, nor a general proclivity, attaching either to the tissues at large, or to the blood, will explain the whole. I might instance many diseases which become quite symmetrical in their development, but which are yet in their origin due to an external and local cause. Scabies is frequently quite symmetrical, and pityriasis versicolor is almost invariably so. Thus there is nothing in the symmetry or in the wide diffusion of acne spots, nor in their limitation to certain regions, which opposes the idea that the disease is in the main aggravated and perpetuated by contagious elements derived from preceding pustules.

May I venture in connection with what has just been said to speculate a little as to the possible nature of that obscurely wonderful malady which we name Psoriasis? The eruption of scaly patches, capable of almost indefinite enlargement and multiplication, which constitutes this disease has obviously little or nothing to do with the general health. Its subjects are always in a good constitutional state, and should an acute disease occur it usually vanishes; to return, however, with returning strength. To a slight extent it is undoubtedly hereditary, yet it but seldom affects several members of the same family. It is not due to any inherited peculiarity in the structure of the skin, for those who suffer from it have usually enjoyed a perfectly normal state of the integument up to the time of its attack. Its development is usually so accurately symmetrical, and we know so well the precise localities which it prefers, that it has hitherto seemed almost impossible to place it in any other category than that of maladies due to constitutional causes. Guided, however, by the explanations which have just been given, we may now assert that these considerations are not in themselves sufficient to justify us in putting wholly aside the possibility of local causation. It is possible that in psoriasis one patch may be the parent of others, as it is obviously the cause of

spreading at its own edge. If, however, we entertain for one moment such a suggestion, it is obvious that we must face the difficulty of hereditary transmission, as well as that of explanation of the origin of the first patch. Only in entire default of a more plausible hypothesis can we venture the suggestion that it may be that some germ, of the precise nature of which we are as yet totally unacquainted, may, in rare instances, pass over from parent to child, and become the cause, when the proper age has been reached, of this most definite and most singular malady. We possess no facts which in the least favour the belief that psoriasis ever spreads from one patient to another. I will not trouble you with any statements in reference to a group of maladies which are clearly and closely cognate with psoriasis, such as pemphigus, lichen planus, and certain forms of nummular eczema. I will take at once a longer step, and adduce for your consideration the facts as regards the contagiousness of Lupus.

Of Lupus, we have two type forms—vulgaris and erythematosus, and, concerning these, the laws as to infective spreading are somewhat different. The patches of lupus vulgaris, as is well known to every one, spread, most persistently, by infection at their edges, but do not usually multiply in number. The suspicion as to blood infection is far greater in the case of lupus erythematosus, in which malady the patches often become very numerous, and are, as a rule, arranged symmetrically; yet these differences, when we look at them closely, are differences in degree, and not such as should absolutely separate one malady from the other. There are cases of common lupus in which the patches are very numerous and placed at great distances one from the other. It is a curious, and I cannot but think, an important observation in reference to these, that the tendency to multiple production is almost always manifested early on in the case and quickly comes to an end. We never think of suggesting to a lupus patient, after once the disease has well declared itself, that he may probably have other patches break out. We know well that the disease will restrict itself to the steady enlargement of those which are present. There is, however, in the very earliest stage, before, it may be, the disease has become well marked as lupus at all, in which it is distinctly inflammatory, and in which there is a definite tendency to general infective spreading. I will adduce a case as an illustration of what I refer to, and then pass on. A little boy in whom whilst in good health an inflammatory patch appeared on one leg, had it quickly followed by a general eruption, which was lichenoid in some parts, pustular in others, and in yet others assumed an almost rupial character. His trunk and limbs were almost covered with spots, but the large majority of these disappeared under treatment, whilst the

sores which had looked like rupia lost their crusts and settled down into patches of non-ulcerating lupus vulgaris. These latter, near twenty in number, have, during five or six years from the onset, continued to spread. Since the first outbreak, however, no new patches have been produced.

It is customary in the present day to consider lupus vulgaris as a tubercular disease, and to attribute its infective spreading to the presence of the bacillus. This may be so, but we must not forget that there are plenty of connecting links between lupus erythematosus and lupus vulgaris; that the former malady often manifests much more active properties than does vulgaris, whilst yet the presence of the bacillus has never been demonstrated in it. The frequent origin of the latter in connection with chilblains or sunburns is, I think, undoubted, and may be cited with many other items of evidence as opposing the idea that the disease is obtained from some external contagion. The remarkable liability of both forms of lupus to be attacked by erysipelatous inflammation must not be lost sight of, since it may possibly supply us with a hint as to the true nature of lupus on the one hand and of erysipelas on the other.

The interest of the lupus family of diseases does not, however, in reference to contagion, end with the two common types which I have mentioned. There are many other most interesting affections of the skin, which are clearly allied to lupus, in that they are capable of indefinite infective spreading. If we are to attribute the various forms of Lupus to parasitic causes it becomes almost certain that the bacilli may remain long latent. There is scarcely ever the history of an injury likely to have been attended by infection.

Concerning this possible latency of infective material, the clinical observer has many interesting facts to supply for the investigation of the histologist. In some cases there is a prolonged, possibly indefinite, period of latency after the reception of the poison before it manifests any effects. The facts are numerous which favour the belief that the tubercular bacillus may remain for many years without producing symptoms. We know also that a patient may have left a leprosy district for many years and have resided during the interval in a country where the malady is unknown, and may then become its victim. In some instances of this late development of leprosy the disease, when once it has begun, runs its usual course without any apparent retardation.

In respect to many of the specific animal poisons which produce exanthematic fevers, we know with tolerable accuracy the duration of the stages which they observe. This is true of smallpox, scarlet fever, measles, chicken pox, and syphilis. In most of these, however, exceptional facts occur which require further investigation. Thus chicken

pox is very apt to vary, within very wide limits, in the length of its stages, and there are remarkable cases in which its eruption may appear over and over again for some months.

Similar phenomena are also not unfrequently seen in Syphilis, but inasmuch as the cases have usually been complicated by the use of a drug which appears to restrain or even to destroy the specific cause our inferences must be made with more caution. In Hydrophobia, however, we appear to have an undoubted example of a disease the poison of which may remain at rest in the tissues for an indefinite time, and then suddenly reveal its presence by the production of acute symptoms due to blood-infection. It cannot be doubted that whilst in some cases symptoms may develop within a fortnight, and in most come on within six weeks, they may in some instances be postponed for two years or possibly even for ten. This fact, so well demonstrated in this very definite disease, is one which we must keep in mind in our investigation of other and more complicated conditions.

The facts are very curious which seem to prove that specific contagia may after the occurrence of an infective outbreak pass into a condition of rest, and then, after a prolonged interval, again manifest their presence and power. Most of them must be regarded as still needing further elucidation and proof. They occur, however, in almost all departments of clinical observation. The recurrence of eruptions of syphilis and of varicella I have just mentioned. Some of us believe firmly that alopecia areata is often a sequel of ringworm, and that it may be produced many years after its predecessor has been apparently quite cured. The phenomena of recurring erysipelas, more especially as almost constantly observed in elephantiasis, are well known to all. In the intervals of these attacks the part is quite free from erysipelatous inflammation. Yet more startling, however, are the cases in which a patient after some trivial operation develops erysipelas, and enquiry elicits the statement, that some years ago the same part suffered from the same disease. I have at present two patients under care who were cured of lupus of the nose by vigorous treatment in early life, and in both of whom, after an interval of twenty-five years, the disease has returned in the same part. It is impossible not to suspect that in such cases some germ-material was left behind in a very inactive condition, which remained undeveloped during the vigour of adult life, but again assumed growth when the advent of senility had weakened the tissues. To sum up, I may assert that we are as yet very far from knowing the limitations of the laws under which infective material may remain latent in the body.

CONCLUSION.

It is time, Gentlemen, that I should attempt to state the conclusions to which the facts I have brought before you seem to point. They are mainly these:—That it is not possible in the case of most of the ordinary forms of inflammation for the process to continue long without being productive of, or attended by, a contagious material; that it is not reasonable to suppose respecting the very various inflammatory maladies which we recognise that they are severally due to accidental infection with some specific particulate virus, destined in each instance to provoke its special train of phenomena; that it is more likely that the inflammatory process takes its peculiarities from the tissues which are first involved, and from the character of the influence which acted as the exciting cause, linked together with peculiarities pre-existing in the patient's state of health. I have not ventured to theorise as to what may be the exact nature of the materies which constitutes the means of contagion in common inflammation. It may or it may not be the fact that it is invariably of a bacillary nature. We are not as yet in a position to dogmatise on this point. Undoubtedly bacillary elements do attend certain of the forms of the inflammatory process which we have been discussing. It may be that elements of this nature are constantly present in our tissues, and wait only for that sort of damage, or disturbance of vital energy, which inflammation brings to become prolific, and thus to assert their force. Whatever may be our ultimate developments of knowledge on this point, I venture to believe that the only safe working hypothesis for the present is that all inflammations, with the exceptions which have been noted, tend to produce a materies competent to perpetuate the process after the pattern of its original type. Erysipelas remains erysipelas, a furuncle begets a furuncle, and an eczema produces eczema. Whilst it may, I think, be reasonably insisted that our principal measures of treatment ought always to be directed to the prevention of the local spreading of the malady, and that, too, chiefly by means of local measures; and whilst I believe that the acceptance of this doctrine would effect a great simplification in therapeutics, I would by no means venture to disregard other and contributory causes. That there do exist peculiarities in the patient's state of tissues and general health, which are not only personal, but may be transmitted by inheritance, and which may very materially influence the course of any inflammation which may be excited, cannot, I think, with any reasonableness be denied. Despite the long-recognised infective qualities of tuberculosis, and the recent discovery of the bacillus, I am far from thinking that the "scrofulous diathesis" has become a fetish. In not a few, however, of the diseases to which I have adverted the element of constitutional predisposition appears to play an exceedingly small part,

and we have to seek an explanation of the malady in the laws of infective inflammation. I have even ventured to extend those laws to another domain, namely, to that of the diseases known as malignant or cancerous. I must not now enter upon this topic in any detail, but will simply say that, respecting a large majority of cancerous diseases, more especially those of external parts, it appears to me that common inflammation usually precedes that of a malignant type. The chief practical difference is, perhaps, that the cancerous process is but very rarely capable of spontaneous resolution, and still more rarely amenable to treatment directed to that end. The infinite varieties of type which it assumes are to be explained in precisely the same way that we explain the different types of the inflammatory process—they depend in the main on the nature of the tissue first attacked. That tissue (as in the instance of melanosis) produces an infective materies by which the process is extended and multiplied. It may be that some form of bacillary growth attends almost all forms both of inflammation and of cancer ; but that there exist specific germs which are the sole causes of the various types of these maladies, and potentially contain their peculiarities, in the same sense as the virus of smallpox does that of variola, seems to me in the present state of knowledge very improbable.

In making these remarks, I can but hope that you will not accuse me of dealing in a merely transcendental pathology—in other words, of beating the wind. On this point I have myself no misgiving, but, on the contrary, feel assured that, whatever may be its explanation, the doctrine of the infectiveness of almost all forms of inflammation is of primary importance in the attempt to develop better rules for prompt and effective treatment.

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SURGERY OF THE TONGUE. By JONATHAN HUTCHINSON,
F.R.C.S., F.R.S., LL.D., *late President of the Royal College
of Surgeons.*

THE surgery of the tongue has been greatly developed within the last five-and-twenty years. I well recollect the time when operations on the tongue were regarded with the utmost dread on account of the fear of hæmorrhage. Removal of the entire organ was never thought of at the London hospitals during the early part of my familiarity with their operating theatres. Even partial operations were rare, and, although so infrequent, I remember witnessing several in which the patient almost died on the operating table. They were never undertaken excepting in cases in which the disease was so far advanced that the diagnosis of cancer was obvious to everyone; and it is needless to add that they were seldom attended by results which offered encouragement to the performance of an operation in itself attended by so much hazard. The return of the disease in the glands was always a matter of certainty and promptitude.

I shall never forget the first case in which I was induced to operate on the tongue. This was in 1858, and about two-thirds of the organ were removed. The case was one of obvious cancer in a man of middle age. I operated in a lodging at Reigate, where I had induced the man to go, partly because I had myself a house there, and partly that he might have the benefit of pure air. Such was my fear of bleeding that I had had constructed two small but powerful screw torsion instruments, by which I was enabled to tighten ligatures which had been passed through, one on each side well behind the diseased part. These were left in his mouth. Having in this way, in the course of forty-eight hours, produced a condition of complete gangrene, I then cut away the offensive mass with

scissors. The plan succeeded perfectly, but it is needless to say that it was a most disagreeable one for the patient. He recovered well, and for some months was in great delight at what he thought the cure of a disease which he had supposed incurable. His well-deserved triumph, however, was not lasting, for in the course of about six months the disease returned in the glands behind the upper third of the sternomastoid. It was impossible to remove them, and after a few months the man died.

In 1858 I was present in the theatre of the Royal Infirmary of Edinburgh, when Mr Syme performed, for the second time, a complete excision of the tongue. It was done before a very large concourse of spectators, and it excited the utmost interest.¹ Mr Syme divided the lower jaw, and held the two halves apart, thus gaining free access to the root of the tongue. He then cut it away, tying the arteries as they were divided. The operation was most skilfully performed, and without any very alarming loss of blood. For a few days it was hoped that the patient would recover, but he eventually sank, and with his death ended, I believe, Mr Syme's advocacy of the operation. It was his second trial of it, both patients had died, and Mr Syme was a man who never put aside the interest of his patients in the procedures which he undertook. He had previously abandoned excision of the knee-joint under very similar circumstances. Not long after Mr Syme's operation we received, at the Pathological Society, from Mr Fiddes,² of Jamaica, a tongue which he had removed for cancer. It was his second case, and the successful issue

¹ Mr Syme's case will be found recorded in considerable detail (by myself) in the *Medical Times and Gazette*, August 14th, 1858.

² The description of Mr Fiddes's operation will be found in vol. xii. of the *Pathological Society's Transactions*, p. 223. He divided the symphysis of the jaw, used a pair of blunt-pointed scissors, and ligatured one lingual before cutting the other. His first case was recorded in the *Edinburgh Medical Journal* for 1859. Both were successful. A report on Mr Fiddes' specimen (by Sir Henry Thompson, Mr Lawson, and myself) records that the cancerous patch was not so large as a shilling, and that the substance of the tongue everywhere else was quite free from infiltration. It was therefore not a case in which, in my present opinion, excision of the entire tongue was necessary.

of both was the means of giving a great stimulus to English surgeons in reference to the operation. Not long after this, Chassaignac's *écraseur* came into vogue, and with it an era of much increased boldness in dealing with cancer of the tongue. This organ, indeed, seemed to be exactly the one best suited for the use of the new instrument, since it is of soft structure, contains no bone, and is peculiarly liable to bleed. The enthusiastic inventor of the instrument had actually gone the length of first fracturing the bones of limbs, and then dividing the soft parts with the chain of which he was so fond. In the case of the tongue no such barbarous preliminary was necessary.

From the date of the introduction of the *écraseur* to the present time we have to record steady progress in reference to operations on the tongue until they have now come to be regarded as amongst the easiest and safest of those which we undertake. Our advance has been in several different directions, and it has resulted not only in a diminution of the risk attending complete excisions, but in a great reduction of the number of cases in which such operations are requisite, and in far better results as regards the subsequent immunity of the patient. We now diagnose early, and operate promptly, with the result that many of our patients remain well for long periods, and more than a few permanently so.

It may be noted as a somewhat unexpected result in reference to the removal of cancer of the tongue, that experience has proved that it is by no means necessary to excise the whole of the organ in cases where the disease is limited to one part. This is contrary to what might have been inferred, for since the substance of the tongue is vascular, succulent, and well supplied with lymphatics, it was only reasonable to fear that infiltration of its substance with infective elements to a considerable distance from the apparent limits of the disease would be very likely to happen. Experience has shown, however, that wide infiltration of the adjacent tissues is not common, whilst, on the other hand, it has revealed the fact that infection of more or less distant lymphatic glands may

be produced by ulcers of the most insignificant size and of the briefest duration. We now know beyond the possibility of doubt that the real risk is of gland infection, and that it begins almost from the very day that the sore assumes suspicious features.

I shall have to advert presently to the very interesting fact that cancer germs may remain latent in the lymphatic glands for periods of several years, and may then, after both patient and surgeon have been willingly believing that the case was a permanent cure, evoke disease. My own experience on the points adverted to has been most definite. Although I am still an advocate for very liberal excisions, I do not now think it necessary to recommend the removal of the entire organ in many cases in which in former years I should, on principle, have insisted on that measure. The cases are indeed but few in which I deem it necessary to remove absolutely the whole organ. They will become, I trust, less and less frequent as years advance and early diagnosis is better understood. With rare exceptions it may be said that the case must have been most grievously neglected in its earlier stages which ever comes to require removal of the whole tongue.

In justification of the partial operations which I have practised and now recommend, I must state that in the whole of my experience of excision of the tongue, I have had but four cases in which the disease returned in the organ itself, or, indeed, in the mouth. One of these was a case of very advanced disease, in which I had removed the whole organ, but in which the tissues were already infected so far back that the disease sprouted again before the wound was well healed. Another was a case in which the floor of the mouth was infected; and in a third, that of the late Earl of——, I had foolishly allowed myself to be influenced by entreaties not to take more than was necessary. With these exceptions my partial excisions (and they have been very numerous) have, I believe, all remained perfectly and permanently sound so far as the scar of the operation and the stump of the organ were

concerned. The picture changes, I am sorry to say, when we come to speak of the lymphatic glands.

My impression is that in at least three-fourths of my cases, sooner or later, the disease has recurred in the glands. In many, the interval has been very considerable, and the gain to the patient from the operation has been very definite. But experience compels the admission that however early the stage at which the operation for cancer of the tongue is performed the probability is very great that enlargement of the lymphatic glands will sooner or later follow. It is to be added to this admission that in but few of the cases in which the glands become enlarged will the surgeon be able to operate upon them with much chance of permanent success. I make it an invariable rule to excise lymphatic glands if they are movable and accessible, and to do it in the freest possible manner; but I am sorry to say I can quote but very few cases in which it has been followed by protracted immunity.

It is an interesting fact in reference to disease of lymphatics beginning from tumours of the tongue that the gland affection may occur in any one of several different regions. The commonest part is in the floor of the mouth, and here, if the enlargement be recognised early, and the patient, having been forewarned of the occurrence, be willing at once to submit to an operation, there is a fair amount of hopefulness. I have at least one patient living in whom a period of at least three years has elapsed since the excision of enlarged glands from the floor of the mouth, secondary to cancer of the tongue. Sir Joseph Lister had operated for similar disease upon a gentleman, with the particulars of whose case I was well acquainted, and who, inasmuch as he died of a quite independent malady eight years after the removal of the glands, and nine after the removal of his tongue, may be deemed to have been permanently cured as far as cancer was concerned.

In the majority of cases of enlargement of glands in the floor of the mouth the patient does not seek surgical advice until many glands are implicated, and until, it may be, they are adherent one to the other, and it is too late to attempt an

operation. Even in some of these cases, however apparently hopeless, it may be justifiable, after fully explaining the facts to the patient, to attempt an operation. If done, it must obviously be carried out in the freest possible manner. Mr Christopher Heath once showed me a very successful case of this kind in which he had operated under conditions apparently almost hopeless.

Another position in which isolated glands every now and then take on enlargement after cancer of the tongue is under the anterior edge of the sterno-mastoid in its upper third. I have several times removed glands from this position. The gland is often a single one, and it may enlarge rather quietly. Provided that it has not acquired adhesions to the sheath of the vessels, and that it be single, a gland in this position is fairly accessible, and its removal is a satisfactory procedure. General M., from whom I removed a gland, full of epithelial cancerous structures, from this position, is still alive and well, five years after that operation and seven after the excision of part of his tongue. If, as they are apt to do, glands in this region have surrounded the vessels, or even become adherent to them, their excision is a difficult, dangerous, and unhopeful procedure.

The worst position of all in which the recurrence of gland disease can take place, and, unfortunately, a not infrequent one, is in the back of the neck behind the mastoid process and the upper part of the sterno-mastoid. I have never seen a case in which I have thought it advisable to attempt an operation for the removal of glands in this position. They usually grow very rapidly, produce a large and very painful mass, and bring about the patient's death after a very short interval. A tumour in this position was the cause of the death of a late very distinguished author. He was near upon seventy years of age, and I had excised about a third of his tongue two years before the enlargement of the glands commenced. There had been no return in the tongue, and during the two years he had enjoyed good health, and been quite free from lymphatic irritation. Both he and I had begun to think that he was secure

against risk of relapse, when, after this long interval, as I have already said, the glands at the back of his neck began to enlarge.

It may be noted as a point of interest that it is very seldom indeed that the lymphatic glands enlarge in these three several positions in the same patient. If a gland tumour appears in one of these regions, it is almost certain that the others will be free. This fact favours the belief that the lymphatic infection is caused by the migration of infective elements in very small numbers, which acquire their location in one or the other part almost as a matter of accident. This inference is obviously, from the theoretical side, a great source of encouragement to the operator. Constitutional tendency to the disease clearly counts for but little. We have to deal with a local malady, and must follow it up perseveringly and courageously by local measures.

In speaking of the circumstances under which operations on the tongue came into general favour, I have mentioned the introduction of the *écraseur* as one of the principal. To a large extent, however, this instrument, once in general use, has now been laid aside. Surgeons have resorted again to cutting instruments. Ingenious methods for securing the bleeding vessels as cut have been devised, or, by some, the precautionary measure of ligature of the lingual, under the jaw, on both sides, has been enforced. For myself, I may state that I have never on any large scale tried any other operation than that by the *écraseur*; nor do I think it probable, judging from what I hear of the statistics of operations by the scissors or the knife, that I shall ever desert the more clumsy—but, as I think, far safer—instrument. I fully admit that my method is tedious, and that, so far from being showy, it has a certain appearance of awkwardness about it. It leaves little or no opportunity for the exhibition of operative dexterity. As a rule, and intentionally, I occupy, by the watch, about half-an-hour in cutting through a tongue. In former years it used to be an hour, but that is, perhaps, scarcely necessary. I also further admit that the wound left by the wire of the *écraseur*, which cuts by crushing,

is somewhat more slow to granulate than one made by scissors or knife, and that it presents for a few days an unhealthy surface. I doubt, however, whether there is any real difference as to the time taken in healing in cases operated on by the different methods, and I feel justified in asserting that the so-called sloughing stage after the use of the *écraseur*—although, perhaps, an inconvenience—entails no sort of danger to the patient. I do this on the strength of an entire absence of mortality, for I have never in my private practice lost a patient after excision of the tongue.

The *écraseur* has, I believe, fallen into unmerited disuse, because it has been chiefly associated with the galvano-cautery wire. During two years or more I was myself in the habit of employing that method. It was a most troublesome procedure, entailing the attendance of Mr Krohne with his battery and duplicate set of instruments, and often risking some burning of the patient's lips. These were inconveniences, but it had one fatal and final objection. Although often absolutely free from hæmorrhage at the time of the operation, it was very apt to be followed by it four or five days afterwards. From this occurrence of secondary hæmorrhage, although I had no actual death, I had several very alarming occurrences. On one occasion, by a most remarkable coincidence, the attack of hæmorrhage began a few minutes before my carriage stopped at the patient's door, and whilst his family medical man, who had preceded me, was sitting by his side. We were able, of course, together, quickly to secure the vessel, but the quantity of blood which had been lost had been very considerable. In another case my late friend, Sir William Fergusson, was good enough to obey a hurried summons in the middle of the night, on account of secondary hæmorrhage in a patient of mine who was at an hotel near to his house. These cases and others—for they were not infrequent—soon induced me to lay aside the galvano-cautery. Since the year 1877 I do not think I have ever employed it for any purpose whatever. It is a very ingenious instrument, but far inferior for all practical purposes to the cold wire. The instrument which I have employed exclusively for the tongue during

the last fourteen years is the ordinary screw *écraseur*, armed with a loop of well-tempered wire. I never employ a chain or any complicated form of apparatus. It is sometimes said that the wire is apt to break, but this has not been my experience. It is well always to be provided with a spare loop in case of accident, but I cannot remember that I have ever myself broken the wire, the explanation being probably that I always proceed very slowly, and allow time for the tissues to give way. At one time I had a favourite loop, with which I had operated in at least fifteen cases; and it was finally broken, not by myself, but by an assistant to whom I had temporarily entrusted the instrument, and who was going too fast.

The details of the method of procedure are the following:—The mouth being kept open by a Smith's gag, the tongue is seized near its tip with a vulsellum and dragged forcibly forwards. It is then transfixed well behind the disease by two strong steel hooks with handles. The vulsellum being then relinquished, the wire loop is passed first over the handles of the instruments, and then over their points, and the nozzle of the instrument being pushed well into the mouth, and care being taken that the wire bites at the proper place, the screw is used rapidly until the wire is well buried in a deep groove. After this the steps are very deliberate. Although I always have a watch placed before me as a security against going too fast, any exact timing of the turnings of the screw is but seldom observed. A little practice makes it easy to guess how to make the whole procedure last about half an hour. When I used to take a full hour there was seldom at the conclusion any bleeding whatever, and I did not usually tie the lingual arteries: now, however, I always expect the latter to bleed feebly when the surface of the wound is rubbed, and make a point of inducing them to do so, and of tying them. Scarcely ever do any other vessels than these require ligature, and the operation during the use of the instrument is quite bloodless.

The after-treatment of the case is usually very simple. The patient is allowed to take milk, or coffee or tea with half milk, from the first, and he is fed by means of a teapot, on the spout

of which a long, flexible nozzle is fixed, which is passed well back into his throat. On the day after the operation the patient is encouraged to leave his bed unless he be old and feeble. There is an advantage in the sitting posture, that it permits more easily of washing out of the mouth. This is to be done very freely, and very frequently. The material used is always the same, a tablespoonful of spirits of wine in a tumbler full of water, either hot or cold, according to the patient's preference. This is the most convenient and pleasantest antiseptic wash for the mouth that I know of. At the end of a week, or ten days at most, the patient is encouraged to go into the open air. Should the granulations appear feeble, and the healing be slow, the process may be expedited by sending him to the seaside.

In exceptional cases the method of operating and the after-treatment above described may require some modification. When the disease is seated very far back, it may be necessary to divide the cheek in order to gain access to it. In the only case in which, so far as my memory serves me, I have lost a patient after excision of the tongue, this procedure had been necessary. The subject of the case was an American dentist in reduced circumstances, on whom I operated in the London Hospital. Erysipelas attacked the cheek, which had been incised, and it probably extended to the wound in the mouth, and death occurred within a week. In other cases I have repeatedly divided the cheek without any ill results. I have never thought it necessary to divide the symphysis of the lower jaw, excepting in one case. In that instance the tumour, a sarcoma, was so large that it blocked the whole mouth, and rendered it impossible to manipulate with instruments.¹ I cut the jaw, not with a saw, but, according to my almost invariable rule, with a strong pair of forceps. The case did well, and the union of the jaw was rapid and perfect. I have found forceps-division of the lower jaw save a great deal of time, as compared with sawing, and the subsequent repair is just as good, or better. In several cases, where removal of the entire organ was necessary, and where it was not easy to drag it out of the mouth, I have

¹ This case is recorded, with a plate, in the *Medico-Chirurgical Transactions*.

adopted the plan of loosening it by a scissors-division of the mucous membrane, and then forcibly pushing the finger under it. In two or three cases, where the patient was feeble and the operation had been extensive, I have thought it wise to introduce a tube through the nose into the œsophagus, and to feed by its means during the first few days. This plan, which I first employed many years ago in a case of cut throat, is very effectual, and may be continued, if needed, for a week, or even longer. A No. 12 india-rubber catheter serves the purpose very well.

In comparing the *écraseur* with other methods of operating for cancer of the tongue, I have not the slightest hesitation in giving it preference as being the one most adapted for general use. For experienced anatomists, fresh from the dissecting-room, ligature of the linguals in the neck has its obvious attractions. Even in their hands, however, it has been occasionally found that, contrary to expectation, the arteries would pour forth blood when divided in the mouth; nor clearly can this operation be commended as one adapted for surgeons generally; it needs great and constant practice for its skilful performance. Operations by means of the scissors, Whitehead's method, &c., are again chiefly suited for those who have a good staff of trained assistants, and they are even in such hands by no means always exempt from very serious losses of blood. To many of those who are the subjects of cancer of the tongue any material loss of blood is a matter of grave moment. These patients are often old and debilitated to begin with, and I cannot but think that it is the hæmorrhage at the time of the operation, and not anything in the condition of the wound subsequently, which is the great source of danger; hence the strength of my plea for the bloodless method by the *écraseur*. Not only is the loss of blood in itself important to the patient, but bleeding into the mouth in one who is under an anæsthetic is always, as is well known, attended by risk that blood may find its way into the air passages. Secondary pneumonia, which has so often proved the cause of death, is, it may be suspected, quite as often due to this accident as to the secretions from the wound. At any rate, the fact remains that the *écraseur* wounds heal well, and do not poison

the lungs. I have come to regard excision of the tongue as a procedure which does not really involve any risk to life.

It remains for me to say a few words in explanation of my statement that I have not in private practice ever lost a patient after an operation on the tongue. I must apologise for the apparent boastfulness which is perhaps involved in my saying this. I make the statement thus prominently in the cause of surgery, and in the hope that it may induce others to adopt a practice which I feel sure is conducive to the saving of life. I am not recommending any procedure in which it may be supposed that personal skill has any share, but one which is so simple that it can be adopted by any one who will take pains, disregard appearances, and act according to rule. I regret much that I cannot count my cases or produce statistics, and my statements must, of course, be discounted on this score to any extent which the reader may think necessary. It would be a labour more than I can undertake to search through the note-books of twenty years, nor if it were done would the list have any claim to completeness. I may state, however, that during the whole of this period I have been a warm advocate of the operation, and that I have adopted it in every suitable case in which I have had the opportunity. Part of my success is, I have no doubt, due to the fact already adverted to, that a large proportion of my operations have been *partial* excisions. Another part is probably to be explained by the fact that I have been a strong advocate for early operations, and have performed many when the disease was but very little advanced. In all these, however, the part removed has been of very considerable size; I have never taken little bits. It is possible, further, that I may have declined a few cases, as being past hope, where others would have operated. It is possible also that during the last five years, during which I have not held a hospital appointment, I may have sent into the London Hospital, to be operated on by others, a few of the most extensive cases. I do not think, however, that there is much to be said on the two latter heads. My strong conviction is, that the difference between my own statistics and those of my friends who use the knife and scissors

is simply that between an operation often attended by very free hæmorrhage (some of it into the lungs), and one which is all but bloodless.

Some lesser matters demand a few words, and I must conclude. As regards sex, I do not think that in the whole course of my experience I have operated on more than six or eight women for cancer of the tongue. Almost all my patients have been men and almost all smokers who had in early life suffered from syphilis. On this latter point my proportions may very easily differ from those of other observers, since many of my patients have been sent to me in the belief that the ulcer was syphilitic.

That the term "precancerous stage" is justifiable, and that there is a period during which naked-eye diagnosis is possible before anything definite is discovered by the microscope, is, I think, well proved by the following fact:—In at least three cases in which different microscopists, who examined the specimen after its removal, reported that there was nothing definite, the disease yet recurred in the glands. I therefore always trust to other signs for the diagnosis. A careful microscopic examination ought always to be made, but it should follow, and not precede, a liberal excision. In one of the very few cases in which I have known the disease return after a partial excision in the tongue itself, most valuable time had been lost before I saw the patient, in the vain hope of establishing a diagnosis by the microscope.

If it be asked whether the doctrine of a "precancerous stage" and the habit of trusting to the aspect, &c., of the sore for diagnosis, do not occasionally lead to errors and to premature operations, I must answer candidly, Yes. A gentleman to whom I had, after consultation with an eminent surgeon, advised excision of part of his tongue, was quite cured by a liberal use of the acid nitrate of mercury as a caustic, and several, under similar conditions, have been cured by the actual cautery. One very obstinate patient, who refused my urgent recommendation that he should have the whole tongue removed, because there were several separate places which looked very suspicious, lived for at least three years afterwards without any malignant

development. This man visited many of my surgical friends, and always expressed his satisfaction that, by disregarding my recommendation, he had saved his tongue. His fate found him, however, at last. He never consulted me again, but one morning a medical friend acquainted with the facts drew my attention to a newspaper notice of his death, and I found on inquiry that he had died after an operation for cancer of the tongue. He would have been a gainer by what some might deem a premature operation, for (let alone the final result) his tongue had been a trouble to him during the whole interval.



ON EDUCATIONAL MUSEUMS,
JONATHAN HUTCHISON, LL.D.

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ON EDUCATIONAL MUSEUMS

BY JONATHAN HUTCHINSON, F.R.S., LL.D.

THE object of the present paper is to advocate the formation in all towns and large villages of collections of objects suited to facilitate the acquisition of general knowledge. Such collections would take a position in comparison with our large metropolitan and provincial museums much like that of the elementary day-school in contrast with a college. When Mr Browning declared, " 'Tis the taught already that profits by teaching," he enunciated both a truism and a paradox. None can know better than those whom I now address, how needful it is that those who wish to profit by their visits to museums should come provided with knowledge. How hopeless is the vacant gaze of the uninstructed as they wander through galleries in which on every side are accumulated objects which would enchain their interest if only they could understand them. If, however, it were exclusively "the taught already" who could gain increments of knowledge, where could be the beginning? The Educational Museum which the present paper contemplates is one which should adapt itself to the untaught and take them in hand from the first. It is one which should in its very existence enter a protest against mere book knowledge, invite all to begin their studies objectively, and become from the first familiar with the things themselves about which they proposed to read. It would aim not much, if at all, at the advancement of knowledge, but take to itself the humbler task of its diffusion. We have had in the past,

and still have, museums of very various scope. The old-fashioned family, or possibly town collection, which comprised all sorts of curiosities, without much regard to their scientific value or otherwise, has nearly died out. They served a good purpose in developing the collectors' spirit, and sometimes in saving from destruction valuable objects; they were also to some extent distinctly educational. From some of these have grown the excellent public collections now to be found in many of our provincial cities, well cared for and well arranged, and second only in size and completeness to those which in our Metropolis are appropriately distinguished by the name of British. Of all these I may say at once that their scope and aim is largely different from those of which I am about to speak. They are collections chiefly for reference by the learned; they aim at completeness almost regardless of cost, and at the fullest development of the most recent science as regards arrangement and nomenclature. That they serve also a most useful purpose in opening the mind of the merely casual visitor, that they offer an admirable field for class teaching, and that they afford us the opportunity for seeing objects of a size, rarity, and costliness which would be wholly unattainable without their aid, we thankfully acknowledge. Still, however, I venture to urge that their primary object is to supply to the advanced student materials for reference, and that their aim is, and should be, completeness. With that aim in view, rather than the instruction of the unlearned public, their very excellence, their size, the countless multitude of their exhibits, and the learned language in which necessarily these exhibits are named and described, all conspire to prevent the attainment of this latter object. All who have seen them must applaud the efforts which have been so successfully made of late years in the noble Museum over which you, Sir, so ably preside, in the direction of making its

treasures more intelligible, and therefore more instructive, to the ordinary visitor. Nor does any one fear that under your supervision this will go too far, but at the same time a possibility must be admitted. No one would wish that the British Museum or any kindred institution should be popularised in any sense which would imply diminished attention to their value as repositories of scientific material. Without desiring to draw any abrupt line, I feel sure that I may venture to urge that merely popular teaching should be done elsewhere. It is this which I claim as the domain of the Educational Museum. It may perhaps be suggested that I am only giving a new name to what is already known as the School Museum. I should be very glad if I thought that such were the fact. Museums to which the pupils have access exist, I believe, at many public and private schools. I am not, however, acquainted with any which has aimed at anything approaching completeness as a means of education. Too often they are merely receptacles for curiosities of the neighbourhood, and of such natural history specimens as the boys may have got together. They are excellent in their way, much better than nothing ; but they do not approach the scope of what a museum ought to be as an instrument in the hands of the teacher.

It is perhaps necessary to say a few more words as to School Museums, and their relation to the topic of this paper. I wish most emphatically to protest that in expressing the wish that every town and every village had its museum, I am not thinking of a collection of local objects. It is necessary to say this, because I find in conversation that almost every one suggests the desirability of this latter object. Now a museum which should illustrate, in an intelligent and accurate manner, the botany, the zoology, the geology, and the past history of any given locality, would require for its formation the services of good

botanists, entomologists, geologists, and so on. A haphazard collection made by half-taught persons would be of little use. The object of an Educational Museum should be to educate, rather than to collect. It might appropriately be associated, when conditions favoured, with a local section, but it would be great error to suppose that a merely local museum could serve the purposes of a teaching one, properly so called. To take but one instance. It is obvious that a museum which contained only local specimens could not teach geology. A man might learn the lias or the chalk, but he must go far from home if he would illustrate the subject as a whole. So with the other branches of Natural Science. Even in botany, which it may be thought could be learned anywhere, it would surely be very foolish on the part of the teacher to neglect the lessons which may be learned from foreign plants. Now a museum worthy of the name of Educational, should refuse no object which can be made to serve the purpose of improving the mind of the pupil. Its scope should be as wide as human knowledge itself, so far as the attainment of that knowledge can be helped by the collection of objects in a room. In a few departments, of which perhaps the knowledge of the heavens may be named as an example, it may be thought that there is but little that a museum can help in. Even here, however, the exhibition of an orrery, a celestial globe, and some good diagrams, might be of great value. In geography, a globe, raised maps, photographs, and engravings of places, and the like, will do much to reduce the fatigue of memory which now too often results from merely learning a class-book by rote. I have purposely mentioned two subjects which may seem to be likely to derive the least aid from exhibits in a museum. When we come to such topics as geology, zoology, and the like, all will admit that collections of illustrative specimens are of the very first importance. Yet even in these, I

believe that we often forget to bring forward much that would be very helpful to the novice. In history, for instance, we forget to show portraits of the men and women we speak about ; in botany, we forget to place within reach of our pupils good drawings of the flowers of the various plants ; and in collections of shells we forget to present illustrations of the living aspect of the animals which inhabit them. It should be the business of the curator of an Educational Museum to remember and supply these omissions.

Amongst the special features which should characterise museums of this kind would be :—

1. The liberal use of pictorial illustration, which should as much as possible be placed side by side with specimens allied to the subjects delineated. For many purposes in teaching, good drawings, or even diagrams, are preferable to the specimens themselves. The latter are of course invaluable for purposes of verification. The skilled botanist prizes his Herbarium, and looks with distrust on drawings of flowers, but the latter are far more helpful to the novice than dried specimens.
2. The plentiful introduction of models and casts. It is enough to allege that by their aid, at a trivial cost, objects may be secured, which for the purpose in view are as useful as their costly originals.
3. Great attention to the preparation of descriptive labels. These should not be mere names, but should point out, in clear and unpedantic language, points in the exhibit especially worthy of observation.
4. Not only should good descriptive labels be abundantly supplied, but it might be well to place in the museum inexpensive works of reference which visitors might on occasion consult.

5. A catalogue should of course be accessible. As to its preparation, a suggestion will presently be made.
6. When the nature of the exhibits permit, the latter should not be locked up in cases, but placed on tables so that they may be handled as well as seen. Many skulls, teeth, bones, shells, stones, fossils, corals, &c., may be so treated, being inexpensive, and easily replaced when necessary. At any rate, it is essential that the teacher should be allowed to handle.

As I have said or implied that it would be well that museums of this kind should be placed within the easy reach of all, that they ought to be found in most towns and villages, at all large schools, and most certainly at all seaside and other watering-places where people are at leisure, it is necessary to make a few remarks on the question of expense. The first requisite for a museum of this kind is plenty of room. This was well insisted upon by our President in the Address to which all listened with so much pleasure yesterday evening. It is a sentiment which every curator will respond to from the bottom of his heart. To him space is of the first importance. However liberal may have seemed at first to be the accommodation afforded, his collections invariably out-grow it. In an Educational Museum, space might be gained, in the first place, by reduction of contents, and by the rigid exclusion of duplicates and objects not essential. There is, however, an end to this which is soon reached; and if a museum is to be clearly arranged, and its contents well displayed, however much these contents may have been cut down, there will remain the demand for large rooms. Now large rooms generally imply much cost. I would try to meet this by great simplicity in architecture. It might be well, under some circumstances, indeed, to build the museum only for summer use, and in the cheapest possible way. Those who

do me the honour to visit Haslemere next Saturday will be shown a little experiment in museum architecture which has not been wholly unsuccessful. I have there a good-sized room which cost very little, which has no artificial heat, and in which yet every object keeps perfectly dry. The style of building must, however, be regulated somewhat by the value of land. In country situations, where this scarcely comes into consideration, I believe that long, low, top-lighted rooms, much like the one which I shall show, will be the best. On that scale a really good-sized museum might be built for £500.

Next as to cost of contents. An Educational Museum need have nothing in it of much pecuniary value. Excellent specimens of all common things may now be bought exceeding cheaply ; nor does this apply only to such objects as skulls, shells, and fossils. Pictorial illustrations of flowers, animals, birds, shells, coins, &c., may now be had of admirable execution, and at marvellously little expense. In a museum which on system should avoid all rarities, and seek only for objects of educational value, the cost of its raw material need scarcely enter into consideration.

Nor if, as proposed, a catalogue and descriptive labels were provided ready to hand, need there necessarily be any expense incurred for a curator. A local committee ought to be able to put such a collection in order, and to keep it so. Their work in its arrangement would be of the utmost advantage to them. I do not in the least underrate the value of a curator ; but obviously, for village museums of the kind contemplated, such luxuries are out of the question.

I will now venture to make a few suggestions as to the manner in which an association such as ours might aid in the development of Educational Museums.

1. It might, under the auspices of a committee, prepare a catalogue which would serve for all museums. It is

a mistake to hold that a catalogue should concern itself only with what is really in possession. It may, without inconvenience, be rather a list of what is desirable, and may thus be made to assist in systematic acquisition. It would be a great aid to a committee in a small town, if a catalogue, printed in good type, and well illustrated, were obtainable, which should designate what objects such a museum ought to contain. Some copies of this catalogue should be printed on one side of the page only, to allow of the type being cut up for labels.

2. In order that the catalogue should not be too bulky, it ought not to enter much into description. The omission to do so should be remedied by the preparation of a large series of labels, suited either for single objects or for groups. These would, of course, be useful for all museums alike, and would save the expense of printing, as well as the trouble of preparation by individual institutions. It is not everyone who can write a clear descriptive, unpedantic label, and of those who can do so, many do not possess sufficient knowledge to be certain of avoiding blunders. A good series of descriptive labels, with the imprimatur of a committee of this association, would, I am sure, be a godsend to the curators of many of our existing museums, to say nothing of the more elementary ones which it is the object of my paper to advocate.
3. After the catalogue and labels would come the preparation of schemata and tabular statements, and possibly of a certain number of pictorial illustrations.
4. I would suggest that a central depot for the supply of type specimens at little more than cost price should be formed. For this object subscriptions to a guarantee

fund would perhaps be needed. I have little doubt, however, that under good management the depot might be made to pay its own expenses.

5. Such a depot as has been suggested might also suitably arrange for the loan of certain objects, and especially of maps, drawings, and illustrative apparatus of a character too expensive to be bought by local committees.
6. I really see no good reason why we should not have moveable museums which might be peripatetic. Why should it not be in the power of a clergyman or a Board School Committee to devote the village school rooms during the summer vacation to the purposes of a temporary museum? It would be easy to supply, from such a depot as has been referred to, an attractive and instructive display of objects, which might be set out and properly labelled in twenty-four hours, and with them a curator competent to give explanatory lectures.

To those who purpose to do me the honour of accepting my invitation for next Saturday, I have to offer a few sentences of explanation. First, I do not invite you to visit my museum so much as to visit Haslemere, where we have much of interest in natural beauty. As regards my museum, I must characterise it as an attempt and not an achievement. You will find it crude and ill-developed in all directions. I have advocated the free use of descriptive labels; but in point of fact my little collection possesses as yet scarcely any. Most of the objects in it are as yet unnamed; and as to the miscellaneous character of its contents, and the almost absolute absence of classification, I fear you will think that I have fallen back upon the model of the museums of two centuries ago. For much of this I make no sort of apology, excepting that I have been my

own curator and have had very little time. The thing has been, in truth, a personal experiment, in which I have been trying, in various directions, how a museum for the instruction of the general public might be inexpensively constructed.

Whilst, however, there is much incompleteness, for which I make no excuse, and which I can but regret, I yet have to allege that I do not think that it is on that account less attractive to my visitors. All knowledge must in the first instance be acquired in a fragmentary manner, and too much of system and organisation repels rather than attracts the unlearned observer. I have always found that my collection affords abundant material for a couple of hours' explanatory lecture, and various plans have been adopted in hope of making it possible for those who visit it unattended, to learn at any rate something. In one sentence, I may say that in comparison with the princely institution over which you, Sir, preside, my museum stands much where a ragged school does in contrast with a university.

In bringing my paper to a close, there are two points upon which I wish to make explanations. The first must take almost the form of an apology to those who have laboured in the same field, for the omission in what I have written of due recognition of their work and names. To have done so would have lengthened my paper unsuitably. No one is, however, more fully aware than myself that there is little, if anything, really original in what I have urged. The formation of museums of the kind to which I have referred has been recommended and even attempted by many. It would be unpardonable if I were to allow what I have just said to cover the omission of the name of my late friend, Mr Toynbee, who wrote a little book in advocacy of inexpensive village museums, and said in it many things which I have now repeated. There is, however, this im-

portant difference between his proposals and my own, that he was thinking of local museums rather than educational ones. The next, really the last topic with which I have to deal, is the objection which I find not unfrequently hinted at, that, in trying to interest all classes in all kinds of knowledge, too much is aimed at. It is suggested that it is better to do one thing well, than many superficially; that any useful grasp of all the sciences is too much, even for the most vigorous mind and the most retentive memory; and that any attempt to make universal knowledge accessible to all is the dream of an enthusiast. There is some truth and much falsity in such suggestions. If the teaching which is proposed were to be from books only, I most fully admit that it would become burdensome to the memory, and even, it may be, confusing to the mind. It is, however, the special charm of all knowledge gained objectively by seeing, and if possible handling the thing concerned, that so far from wearying, it is a refreshment and a pleasure. The memory retains such knowledge without effort, and recurs to it with delight. Now, the aim of the museum-maker should be to dethrone pedantry, and offer the very things themselves to the contemplation of all. In this way only, and so far as I know, does sound knowledge become possible. I have not one word to say against the minute and detailed study of particular subjects by individuals; such specialisation is an absolute necessity to the advance of science. But ninety-nine out of every hundred of those for whose enlightenment the educational museum is designed, would never undertake any one subject; and surely, even to the specialist, there must be both pleasure and profit in general knowledge. Is it not somewhat a reflection upon the intelligence of the nineteenth century, that here, at its end, there should be found those who are as willing to acknowledge ignorance of geology as of horse-racing, and

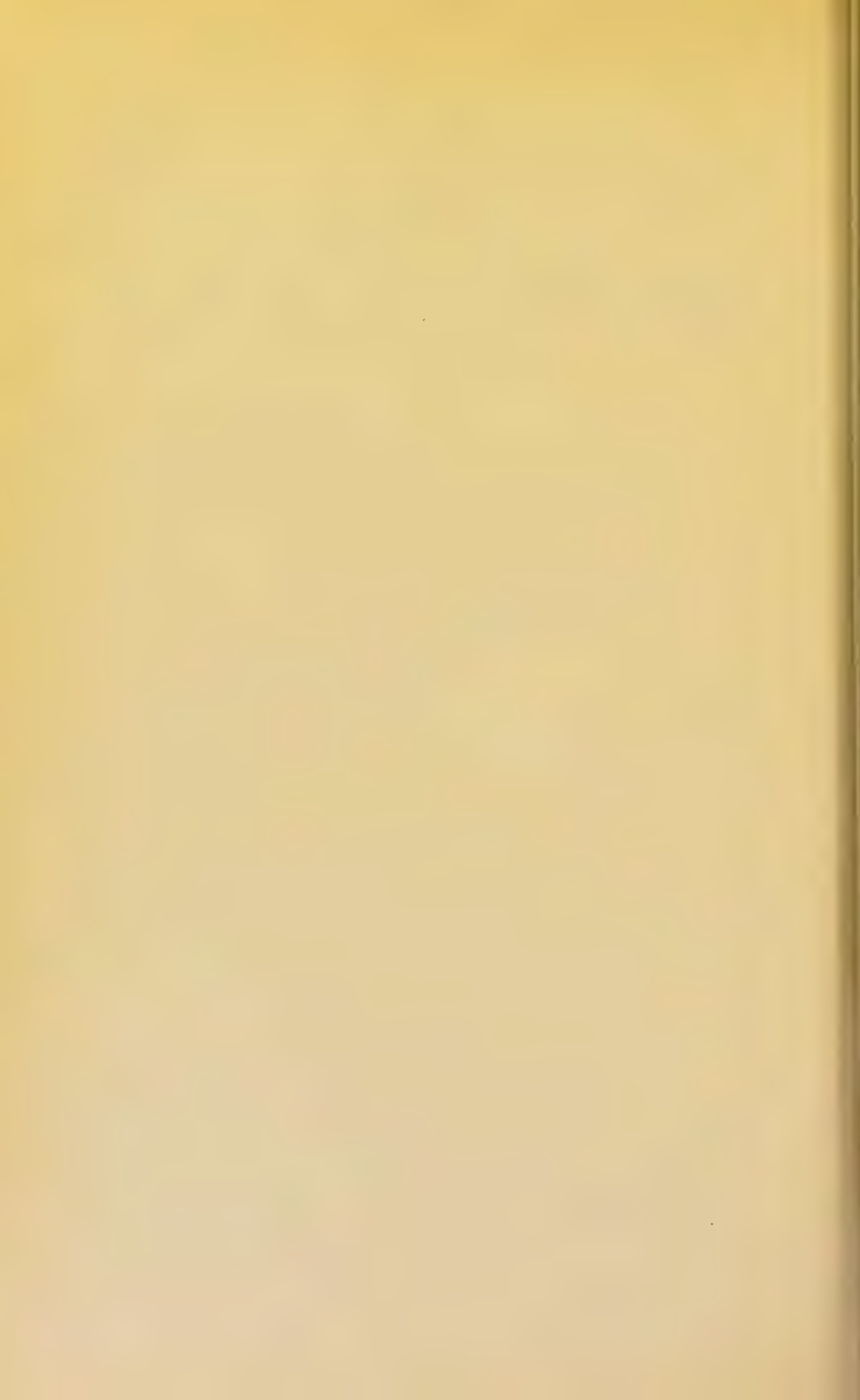
who would speak of a zealous palæontologist much in the same tone as they would of a collector of postage-stamps? Surely the great facts as to the history of the formation of our planet are essential to education. They are at present shunned by most through a mistaken estimate of their difficulty. Undoubtedly, geology in its details is full of complexities and hard words, and in this it only shares with all the sciences. It is, however, as true of it as of them, that the fundamental facts may be taught easily to all, and if taught objectively may be made a pleasure. What Wordsworth so well asserted of the essentials of the Moral Law is true also of those of Science:—

The primal duties shine aloft like stars,
The charities that soothe, and heal, and bless,
Are scattered at our feet, like flowers.

I speak from some little experience when I aver that museum-teaching may easily be made to interest all classes. Whether, however, we regard the acquaintance of a general knowledge of natural facts and of the history of nature as easy or difficult, I unhesitatingly assert its priceless value, and, apart from its collateral advantages, its absolute necessity. It is that which will make our lives cheerful and rational; it will enlarge our sympathies, diminish our superstitions, and fit us for our duties one towards each other. Nearly four centuries ago, Erasmus wrote concerning the Bible as follows:—

“I wish that even the weakest woman might read the Gospels and the Epistles of St Paul. I wish that they were translated into all languages, so as to be read and understood, not only by Scots and Irishmen, but even by Saracens and Turks. But the first step to their being read is to make them intelligible to the reader. I long for the day when the husbandman shall sing portions of them to himself as he follows the plough, when the weaver shall hum them to the tune of his shuttle, when the traveller shall while away, with their stories, the weariness of his journey.”

The noble desire expressed in these words has since then been nobly achieved, and the Bible is now in the hands of all. The need of the present time is, that the same should be done for the Book of Nature, and to the display of its pages a liberal extension of Teaching Museums seems to me the first necessity.



Chol. Am. Society for Ind.

ON
URINARY CALCULI:

THE LESSONS WHICH THEY TEACH AND THE
PROBLEMS THEY SUGGEST.

COMPILED BY
JONATHAN HUTCHINSON, F.R.S., LL.D.

*Being the Essay appended to the Tenth Fasciculus of the
New Sydenham Society's 'Atlas of Pathology.'*



THE illustrations which are given in these Plates have all been copied, by special permission of the Council of the College of Surgeons, from those published in its Catalogue. The freest possible use has also been made of the descriptions which accompany them, and of the information afforded by the introductory remarks. As in many instances the phraseology has been changed and abbreviation attempted, it has not been found possible to introduce quotation marks so often as would otherwise have been desirable. I must therefore be content with this general acknowledgment of indebtedness.

Urinary and other calculi offer exceptional attractions both to the collector of specimens and to the pathological artist. Every operating surgeon has his private series, and most of our museums contain them in great abundance.

Recent improvements in surgery have, however, probably put an end to the possibility of obtaining these concretions in the future, and in the course of another century the extrinsic interest of antiquity will be added to our collections of calculi, whilst new museums may find it difficult to obtain series of specimens. The ease and safety with which all stones of less than unusual size are now crushed and got rid of will make it less and less common for patients to delay treatment until extraction, without crushing, is the only resource.

The classification of calculi, which by some authors has been carried out in much detail, may for clinical purposes be greatly simplified. The attempt to divide those of the lithic acid group into subdivisions according to the element which constitutes the nucleus, although of a certain amount of interest in reference to the mode of formation of the nucleus, does not at the present stage of our knowledge serve much useful purpose. We now know that uric acid, urates, or oxalate of lime may any one of them constitute the nucleus, and that after once this has been formed the subsequent accretions may be of any one or of all, in turns or together, according to the varying states of the patient's urine. We place, then, in one large family all stones in which these closely-allied salts are found, without attempting to register too minutely the precise order of their relation one to another. The laws under which this relationship determines

the order of the different layers are of great interest, but they may now be discussed without encumbering ourselves by too much of statistical detail of the kind referred to. The uric oxide calculus, so rare as to be little more than a chemical curiosity, takes its place in the same large group, and so must also, though with a longer interval, that composed of cystine.

Thus then we may conveniently, for clinical purposes, place in one large group all stones which contain uric acid or oxalate of lime, or their allies, in any part of their substance, and it is clear that in this group we comprise almost the whole of what are known as primary calculi.

Another group must be constituted of those stones which consist of phosphates, and in respect to these some questions yet unsettled will have to be entertained. We may admit at once that the majority of phosphatic concretions are what have been called "secondary," that is, consequent upon some diseased condition of the organs in which they are formed. Probably a very large number of the stones classed in our museums as "phosphatic" are "secondary" in a yet stronger sense, in that they have nuclei of some other salt. It remains, however, an open question whether some of the harder forms of phosphatic concretion have not a fair claim to be regarded as primary, and if so, the phosphatic group should be divided into two. It will naturally encounter a further subdivision as regards the kind of phosphate, whether bone-

earth, triple, or their mixture the fusible salt; but, as just explained in respect to the representatives of the lithic acid family, no great clinical inconvenience will result from placing them all together in the first instance.

Our last group will contain the cases in which calculous concretions have formed round bodies which have been introduced from without, and these, excepting that they afford us instructive illustration of some of the laws of urinary deposition, have but little interest for the medical investigator. They stand wholly apart from the other groups, and teach us little or nothing as regards diathetic states. The problems which they offer us are comparatively simple, since they are always preceded by the occurrence of cystitis. With them we may also place certain pseudo-calculi, such as the urostealith (around soap), fibrinous calculi, and blood concretions.

SALTS WHICH ENTER INTO THE COMPOSITION OF CALCULI.—Amongst the ingredients which may enter in the composition of urinary calculi, the following must be mentioned as the principal. Most of these salts may occur either in the amorphous or crystalline form, and often in various combinations, or admixture with each other.

Uric Acid.—One of the commonest of urinary deposits. It occurs at all ages, as the well-known red-sand, in grains easily visible to the naked eye, and which have been aptly compared to cayenne

pepper. Lithiasis, or the presence of free uric acid as an easily recognisable deposit, is far more common than the occurrence of calculus, and may exist for years, especially in young persons, without any formation of either gravel or stone. It is often seen in large quantities in the pelvis of the kidney, and it may form a concretion either in the kidney or the bladder. It is very often accompanied by a varying deposit of lithates. It may vary much in depth of colour, but is always of some tint of yellow or brown.

An almost pure uric acid is the calculus of adults. In elderly men no other primary calculus is in the least common. In young persons oxalate of lime is frequent, and, although uric acid is not at all rare, it but seldom maintains its individuality, being almost always either alternated, or mixed, with other salts. A large, dense, heavy stone, consisting of uric acid throughout, may be taken to have been almost certainly removed from a patient of at least middle age.

The quantity of uric acid sand passed by some patients is very large indeed. It may be continuous over long periods, or may occur in sudden and possibly in periodic attacks. Precisely similar statements are, I think, true of those who pass phosphatic sand, and it would appear that both are dependent upon conditions of assimilation which are susceptible of rapid and sudden changes. The following statement by Dr.

Henry is important, although we cannot help suspecting a misprint in the first part of it, and that for "ounces" we ought to read scruples:—

"Two instances have fallen within my knowledge, in which persons have voided quantities of uric acid with the urine, far exceeding anything that I can find upon record. In the first, which was mentioned to me by Professor Memo, of Edinburgh, every pint of the urine voided by a man about forty years of age, who laboured under symptoms of gravel, deposited about two ounces of a brick-coloured sediment, which I found on examination to be chiefly uric acid, with a very small relative proportion of the earthy phosphates. In another instance, a lady of middle age, who was subject to gravel, was in the habit, when warned of its approach by the usual symptoms, of having recourse to a medicine, the composition of which is kept secret, but which appears to me to be nothing more than spirit of turpentine coloured by a little petroleum, with the addition of a portion of tincture of opium. The uniform effect of this medicine was the discharge of a sandy substance in such quantity, that more than four ounces were sometimes voided within the space of two or three days. It was composed chiefly of uric acid, with a small proportion of urea and of the earthy phosphates. I have since known another instance in which the same medicine has produced a similar effect, though not to an equal extent."*

* 'Henry on Urinary Calculi,' *Medico-Chirur. Trans.*, vol. ix. p. 137.

The Uric Oxide Calculus.—The uric oxide or xanthic oxide calculus is simply a variety of the uric acid, from which it differs in containing merely a few more equivalents of oxygen. It is of interest rather to the chemist than to the physician, for it is produced probably under precisely the same conditions as those which favour the formation of the more common type. Yet, although so close an ally, it appears for the most part to preserve its individuality, and the two salts do not usually mix. Very few instances of its occurrence are as yet on record.

Dr. Marcet, who first described this compound, had in his possession a calculus which weighed only eight grains. It was of a reddish cinnamon-colour, its surface was smooth, its texture hard, compact, and laminated. Of the history of this stone nothing is known, and no portion of it survived the chemical analysis.

Another calculus of the same constituents was examined by M. Langier, and recorded in the '*Archives Générales*,' vol. xxi. In this case also the stone was very small.

The third example was examined by Liebig and Wöhler, who confirmed Dr. Marcet's discovery, and carefully determined the elementary composition of the substance. In this instance the stone was as large as a small hen's egg, and from centre to surface of the same constitution.

I have abstracted the above statements from the College Catalogue, and must refer the reader

to it (or to any good treatise on the subject) for details as to the mode of analysis.

Urates.—The urate of ammonia is the most common, but the urates of soda, potash, lime, and magnesia also occur. The urates constitute the well-known brick-dust or lateritious deposit so frequently seen after unusual exertion attended by perspiration, after any febrile attack, or in connection with indigestion or unsuitable food. “Muddy urine” is the popular expression for that which contains lithates, and many persons know well by what articles of food,—beer, wine, &c.,—their water may be made muddy. The urates are soluble in warm fluid, and in large quantities of water when cold, and they are very rarely to be seen at the time the urine is voided. Hence the explanation of the infrequency with which they constitute calculi. Although, according to most authorities, not usually forming the nucleus, they are deposited with tolerable readiness when once a nucleus is formed. They frequently alternate with uric acid, or occur in admixture with it. They may also be mixed with the phosphates. The greater the degree of concentration of the urine the more likely it is to deposit urates; thus they are apt to occur after long fasting. As a rule the urates are deposited as an amorphous salt, and it is very common to find the different bases mixed together.

The urate of ammonia calculus is usually met with in children, and never attains a large size. Respecting them Mr. Taylor writes:—“I must

fully concur with Dr. Prout that all those calculi which present when broken an amorphous and earthy-looking fracture consist essentially of urate of ammonia." Urate of ammonia is frequently met with in the fusible calculus.

When the urates occur in a crystalline condition it is under somewhat different circumstances. Sir William Roberts has drawn our attention to the important clinical fact that the crystalline urates are deposited within the urinary passages, and may thus easily constitute the nucleus for a calculus. He suggests that the great comparative frequency of vesical calculi in children may not improbably be due to the fact that the crystalline urate of soda very frequently occurs in the febrile attacks to which they are subject. This salt forms what are called "hedgohog crystals," from which sharp spines project, and when complaint is made in children that the passing of the urine gives pain, this is the salt which should be sought for. The admixture of the urate of ammonia with the phosphates in calculi is to be explained by the fact that this salt is liable to be precipitated when the urine becomes strongly ammoniacal, if there be at the same time a tendency to excess of uric acid.

Oxalate of Lime.—The oxalate of lime usually occurs as a colourless deposit, which may easily escape attention. If the urine be placed in a glass, numerous fine lines are produced, giving it the appearance of having been scratched; and, on careful inspection, these lines are

seen to be caused by minute white crystals. The oxalate is insoluble in water and in the vegetable acids, but may be dissolved by the mineral acids. It occurs very frequently in conjunction with uric acid and the urates. The influence of food, and especially of certain kinds of vegetable foods, such as fruits, rhubarb, &c., in throwing down oxalate of lime, is well known. Conditions as regards derangement of digestive power similar to those which produce uric acid, may, under slight alterations of diet, produce the oxalates. Dr. Owen Rees even held the opinion that the oxalate of lime never exists free in the blood, but is produced from uric acid after separation from the latter.

That the oxalate of lime calculus is not always of dark colour is illustrated by figs. 4 and 5 in Plate XXXI. In these instances, which certainly at first sight would not be taken for oxalate of lime at all, the salt is in a crystalline condition, and the texture of the stone is loose. Although not of dark colour, they do not afford good examples of "the *white* oxalate." Of this a fair instance is afforded in fig. 6 of Plate IX. of the College Catalogue (not copied). In this the surface of the cut stone is smooth and hard, and looks like a slightly pink porcelain. It is stated by several authors that in the Norwich Museum are specimens of a yet whiter tint, but I have failed to find them. When oxalate of lime is deficient in colour it is almost always from the stone having been formed in the kidney.

Its usual colour is derived probably from the urine.

It is a remarkable fact that oxalate of lime calculi, although both heavy and rough, are often attended by but slight symptoms of irritation. Mr. Poland, in Holmes' Surgery, has figured a stone of this kind of considerable size which he had removed from the bladder of a boy of twelve, who had shown no symptoms until two weeks before the operation. I once myself removed from the bladder of a boy a stone of this kind, which was unusually heavy and rough, but in whom the symptoms had been almost absent. Put to the test of jumping from a chair, he did so with alacrity, and made not the slightest complaint.

Cystine or Cystic Acid.—This is a crystalline body, which has only as yet been detected with certainty in the urine of man, the pig, and the dog. Nothing is known as to the special conditions of health which produce it, and it is seldom recognised in the urine, excepting when the symptoms of a calculus reveal its presence. When once attention has been drawn to its existence it is, as a rule, found to be constantly present in greater or less quantity. Periods of intermission may, however, occur. The urine which contains it is generally slightly turbid, of a yellow colour, and with a peculiar "sweet-briar" odour. The composition of the urine in other respects is usually normal, as it is indeed in most other calculous states. It is the

calculus-forming material itself which is alone in excess. Cystine is remarkable for containing sulphur. The peculiarity that by long exposure the yellow tint is changed to green is observed both in the urine which contains cystine and in section of the calculus itself.

Xanthine or Xanthic Oxide.—This is a very rare urinary deposit, closely allied to uric acid. It occurs as a granular deposit, which is soluble by heat, but with considerable variations. It is not impossible that mistakes have occurred in more than one of the records as to its occurrence as a deposit, but there is no doubt that it has been encountered as a calculus six or eight times. It has been met with in Germany, England, and India. The subject of the Indian case was a Mussulman child, and the fact is of importance as illustrating the sameness of urinary concretions in different races. Wherever uric acid calculi are common, there we must expect now and then to meet with one of xanthine.

Carbonate of Lime.—This salt is the common deposit in the alkaline urine of the herbivora, and forms their calculi. It is not unfrequently precipitated in human urine in an amorphous condition, together with the phosphates, when ammoniacal decomposition has occurred. As a primary deposit, whether in the form of sand, gravel, or calculus, it is extremely rare. In a few cases patients have voided numerous small calculi composed of carbonate of lime, or such

have been found in the bladder or kidney; but in such cases the suspicion is always to be entertained that some local obstruction exists, and that the salt has been deposited in connection with a cyst in the prostate, bladder or kidney rather than directly from the urine. Sir W. Roberts admits the carbonate of lime calculus to the rank of one of the primary ones occurring in the human subject, though of extreme rarity. In one of the cases which he quotes, however,—that recorded by Mr. Wagstaffe,—it has since been proved that there was a mistake in analysis. It is certainly curious that a deposit which is common in the herbivorous domestic animals, and which in them produces large primary calculi, should in the human subject be all but unknown. The Hindoo is as nearly as possible a vegetarian, but his calculus is like that of a flesh-feeding Englishman, almost always of the uric acid group, never carbonate of lime.*

The Phosphates are usually met with mixed, but it is very important that their several varieties should be considered separately.

Phosphate of lime, basic or amorphous phosphate of lime, bone-earth, is invariably precipitated in alkaline urine. If the alkali which

* Concerning the carbonate of lime, Dr. Ord writes: —“Many circumstances point to the conclusion that where carbonates are formed and enter in the composition of calculi from the urinary organs, such calculi have been formed out of the course of the urine, in diverticular passages where other fluids than urine would predominate.”

precipitates it is a fixed alkali, it may be the sole deposit, but as ammonia is by far the more common cause of its appearance, it is usually accompanied by the triple phosphate. Heat throws down this salt when in excess, as is well known to all accustomed to use heat as a test for albumen. It is soluble in acids. "Its occurrence depends simply in the existence of an alkaline reaction, and the presence of lime and phosphoric acid in the urine" (Roberts). It is the normal deposit of urine alkaline after a meal, or of that made alkaline by medicinal treatment. It may be present when the urine is voided, in sufficient quantity to cause turbidity, and, if so, the deposit does not increase on cooling. When cystitis occurs bone-earth is secreted by the mucous membrane of the bladder (Brodie and Prout). Bone-earth rarely constitutes the nucleus of a stone, for it appears, in consequence of its non-crystalline condition, to concrete with difficulty. It is, however, very common in combination with the ammoniaco-magnesian salt.

There is a crystallised form of phosphate of lime to which attention was first directed as a urinary deposit by Dr. Hassall, and which was subsequently investigated by Sir W. Roberts. It is a primary deposit, and is met with in association with oxalate of lime, and even with uric acid ; usually in those who are seriously out of health. This salt might easily constitute the nucleus of a stone, but it is rare.

The triple phosphate—magnesian or ammoniaco-magnesian phosphate—is a crystalline salt insoluble in water, and of a white colour. It often constitutes what is known as “white sand.” It is usually met with in association with phosphate of lime constituting the fusible calculus. It may, however, occur alone, may form the nucleus of a renal calculus, and produce its chief bulk (see page 8). Its usual cause, or, according to some authorities, its invariable cause, is the ammoniacal decomposition of the urine. There is reason to believe, however, that it may occur without decomposition, or any appreciable inflammation of the urinary passages. Sir W. Roberts places it unreservedly in the group of secondary concretions, but the evidence as to its right to a place amongst the primary ones is at least as strong as is that of the carbonate of lime. It has been found in the kidney constituting a large stone in cases in which there was no history of preceding disease. The more nearly pure the salt the greater the probability that it is due to constitutional rather than local causes. Sir W. Roberts himself records (p. 100) a very instructive case in which it appeared in the urine under circumstances closely fitting with Prout’s description of the “phosphatic diathesis.”

PHOSPHATIC CALCULI AND THE “PHOSPHATIC DIATHESIS.”—The old term “phosphatic diathesis” is now almost wholly disused, and very properly so if

it were to be employed, as formerly, to include all conditions in which phosphatic depositions occur. We now know that in a very large majority of cases the phosphates are simply the result of decomposition of the urine and of inflammation of the bladder, and do not in any way denote such peculiarity of the patient's health as might deserve the name of a diathesis. This statement, however, concerns the mixed phosphates, and more especially the triple or ammoniaco-magnesian salt, which is always one ingredient in the mixed (or "fusible") concretion. When, however, a calculus contains none of the triple salt, but consists of bone-earth only (phosphate of lime), its significance is altered, and some further consideration must be given to the question as to whether the state of the patient's health may not have something to do with the formation. The same suspicion may also be entertained in certain rare cases in which the triple phosphate is present in a state of purity. The concluding sentence of Sir W. Roberts' summary on this subject, "the phosphatic diathesis of Prout is simply ammoniacal urine," is, I cannot but think, too strongly put. There certainly are cases in which phosphates are freely deposited which have nothing to do with ammoniacal decomposition of the urine.

In connection with this question it is of great interest to enquire whether there are calculi which consist wholly of bone-earth (phosphate

of lime),* or of a pure and unmixed triple phosphate, and, if there are such, to ask under what conditions they are formed. In the Introduction to the College Catalogue it is stated that the ratio which calculi consisting entirely of the phosphates bears to the whole number is as 1 to $13\frac{1}{2}$,† but in a previous sentence it has been said “there is no instance in which the phosphates form the nucleus of a calculus”; so that it is clear that we must take the expression “consisting entirely of the phosphates” with some limitation. We must remember also that by “phosphates” is here meant the mixed salts, for at page 103 we find that “calculi consisting entirely of phosphate of lime,‡ having an un-

* Mr. Taylor (1838) objects that the term “bone-earth” is not closely applicable to phosphate of lime as met with in calculi, since in the latter the portion of acid is much larger than in the lime of bones. The proportion of acid, he points out, varies a good deal in different specimens of calculus.

† Sir Henry Thompson, our chief English lithotritist, in a report in the 53rd volume of the Medico-Chirurgical Society's Transactions, recorded 184 consecutive cases treated by crushing. As regards the kind of calculus, it was believed to have been uric acid or urates in 122 cases, mixed in 16, phosphatic in 40, oxalate of lime in 4, cystic oxide in 1, and pure phosphate of lime in 1. The rarity of the oxalate of lime is probably explained by the fact that all the patients were adults. It is possible that of those called phosphatic not many were wholly or primarily such.

‡ Dr. Ralfe exhibited, at the Pathological Society in 1880, some calculi from the kidney which consisted of phosphate of lime, with excess of organic matter, and mere traces of uric acid. The patient was a foreign

doubted renal origin, are of extremely rare occurrence. There is no specimen of this description in the Collection, nor does it in any instance form the nucleus of a calculus." This strong negative fact is, however, possibly not so conclusive as it may at first sight seem, for since it is admitted that as a "principal constituent of urinary concretions it is very common," it may be simply that it has great difficulty in forming a nucleus, even when superabundant in the urine and quite capable of forming a concretion when once a nucleus is afforded to it. We must bear in mind that phosphate of lime is not only an ingredient in healthy urine, but that it is possibly formed by secretion from the mucous membrane both of kidney and bladder (and some other mucous surfaces). We have therefore to be on our guard lest we mistake the phosphate of lime due to cystitis for that resulting from its excess in the urine. That there are persons who habitually pass this salt in great excess, through long periods, cannot be doubted, and that too without any disease of bladder or kidney, and often with apparently fairly good health. The question is whether in such patients, for whom, perhaps, the term "phosphatic diathesis," in the sense originally in-

seaman, who had died after severe symptoms of renal irritation, &c., following a blow. The case is proof that phosphatic calculi may form in the kidney. The injury occurred only just before the fatal illness, and had nothing to do with the formation of the stone.

tended by Prout, may safely be still employed, we ever encounter the formation of primary calculi, or of calculi formed so almost exclusively of this salt that we must consider the constitutional state to have taken a share in the result such as it does in the formation of uric acid stones. The evidence given by the College Collection is, as just stated, to the effect that phosphate of lime never at once constitutes the nucleus and the entirety of a stone.*

Dr. Wollaston described, in the 'Philosophical Transactions' for 1797, a concretion of this kind, which had been given to him by Dr. Baillie, but unfortunately no particulars are recorded as to the patient from whom it was obtained. It is said to have been of a pale brown externally, and so smooth as to appear polished, whilst its section showed regular laminae, which adhered but slightly.

Mr. Thomas Smith, in 1870, presented to the Pathological Society, on behalf of Dr. James Wise, of Decca, Bengal, thirty-one tetrahedral stones, which together weighed nearly five ounces. They had been removed by lithotritry from a

* "Phosphate of lime has in two instances been observed by Dr. Prout to be deposited from the urine as an impalpable powder in very large quantities, and of considerable purity, when no disease of the bladder was present."—College Catalogue. I have myself seen several such cases, but in no instance did a patient ever bring me phosphatic gravel which could be supposed to be primary. Probably the amorphous phosphates form concretions with great difficulty.

Hindoo peasant, aged thirty-five, who recovered well and soon resumed his work. By the analysis of Dr. Mathieson, one of the stones was determined to be phosphate of lime. Sir Henry Thompson has recorded a case in which he removed, by crushing, a large stone which is said to have consisted solely of phosphate of lime. The patient was aged about thirty-five, and had suffered from disease of the bones of the spine in early life. Four years previously he had voided two small calculi said to be phosphatic.

In fig. 2 of Plate XXX. we have an instance of what is called in the Catalogue a "confirmed phosphatic diathesis." A large stone is seen, which is of laminated structure and compact substance, excepting a thick encrusting layer. This outer layer is of the ordinary loose mortar-like substance of the mixed phosphates so constantly met with on the exterior of all stones which have caused cystitis. It is, however, totally different in texture and aspect from the bulk of the stone, although both are white. The difference between the true calculus and its encrustation is almost as great as between a uric-acid stone and its coating of phosphates. Yet in this instance both the calculus and its crust rank as phosphates. The nucleus of the stone is a small irregular lump of uric acid, and alternating with layers of amorphous mixed phosphates are narrow lines of yellow lithates. Here, as in some other instances, the suspicion seems plausible that the stone was the result

not of cystitis and urinary decomposition, but rather of a urine kept alkaline, or nearly so, for a long period by artificial means. At length cystitis did supervene, and then there came the encrusting mortar-like layer of crystalline triple phosphate. The existence of the uric acid nucleus makes it certain that the patient had not in the first instance "a phosphatic diathesis." That such a condition existed through a very long period is made probable by the large size of the concretion, its firmness, and its lamination, and that it was induced artificially seems only a reasonable conclusion. Unfortunately it is a British Museum (or Sloane) specimen, and consequently there is no forthcoming history of the patient from whom it was obtained. It may possibly be from one of the many who, after the disclosure of Mrs. Joanna Stevens' "discovery,"* swallowed alkalies in enormous quantities for long periods in the hope of dissolving their stones. It is clear from the alternation of the urates with the phosphates that some difficulty occurred in the maintenance of the alkalinity of the urine.

I have searched not a few museums and museum catalogues carefully for examples of the phosphate of lime calculus without other

* In the year 1750 Mrs. Stevens received from the Government five thousand pounds for a secret remedy which was vaunted as efficient in the solution of stones. It consisted chiefly of lime obtained from the shells of eggs. Its disclosure led, we are told, to an extended employment of alkalies for this purpose.

nucleus or admixture. I have been able to find, however, exceedingly few instances, and it is to be regretted that in none of them are the case-histories sufficiently complete to allow of our obtaining from them much clinical knowledge as to diathesis. In some of them, however, there does not appear any reason to believe that the formation of the stone was preceded by cystitis.

In the College of Surgeons' Museum, No. II. 6 (a Hunterian specimen), is the half of a calculus composed almost wholly of phosphate of lime, but with a little of the triple salt. H. 43 is a similar one, "from a man aged 63 in St. George's Hospital."

Although the alternation of layers of other salts is not common after once the deposit of phosphates has commenced (Marcet's law), yet it is to be observed that the admixture of urates with the earthy phosphates occurs not very infrequently. This is of interest, as proving that the states of health permitting of the deposit of these two salts are not in all cases widely separated, or, in the older phraseology, that the two diatheses may even coexist. Thus H. 35 is a calculus in fragments, which weighed six drachms, and consists of earthy phosphates, with a large quantity of urate of ammonia. In H. 47 mixed calculi, containing both phosphates and urates, were finally coated by a thin layer of urate, the patient being an old man of 74. H. 32 is remarkable on more than one account,

for the patient was a negro (a race almost absolutely exempt from stone), and the calculi had a crystalline triple phosphate nucleus, with a mixture of fusible salt, urates, and uric acid externally. There were twenty-one in number.* The patient was a negro boy of four years old living in Demerara. There can be little doubt, having regard to the nucleus and to the statement that they were removed from the urethra, that these calculi were not primary, but had formed in connection with some disease of the organs attended by impediment to exit of urine. In H. 18 we have another instance in which the fusible compound is mixed with urate of ammonia; and the same statement is true of H. 19, H. 20, and H. 21. It is clear, then, that we must not allow the creed that the formation of phosphatic deposits once begun supersedes and prevents ever after the deposition of uric acid or urates, to obtain without a free allowance for exceptions.

I have already alluded to the fact that when calculi are composed wholly of phosphates they are almost invariably found in association with disease of the urinary organs. Some of the best examples occur in connection with paraplegia, others with stricture or enlarged prostate, or with organic disease of the bladder.

* "An old negro woman in Demerara made an incision into the urethra of a negro boy four years of age, and extracted these calculi, and made a perfect cure of her patient. James Hendy."—Hunterian.

The College Catalogue contains a list of forty-eight specimens of calculi of this formation. In most instances it is expressly stated that the stone consisted of the fusible phosphates, and, as no information is given as to any nucleus, it is to be presumed that none can be discerned. In a large majority of these forty-eight specimens no history whatever of the patient's case is appended, none having been recorded by the donor, and we can therefore make no use of them in reference to the point under discussion. In a few, however, the history is forthcoming, and remarkably illustrates the general statement that phosphatic concretions are usually secondary to inflammatory mischief induced by some mechanical cause.

One of the best of these, and with the most complete history, is that numbered 2. In this instance the stone had attained the enormous size of sixteen inches round its greatest circumference and fourteen in its smallest. It weighed forty-four ounces troy. It consists of the mixed phosphates. An attempt to remove was made by Mr. Cline a short time before the patient's death, but it was found to be so hard that it could not be broken up, and its bulk made it impossible to think of extraction. The history of the case was that the sufferer (Sir Walter Ogilvie, Bart.) had twenty-eight years before his death suffered an attack of paraplegia in connection with a blow upon the spine, which was probably attended by fracture. For two

months his lower limbs were paralysed, and the catheter was needed. After that a slow recovery followed up to a certain point, but was never complete. The stone was first recognised by sounding by Mr. Benjamin Bell, of Edinburgh, twenty years after the accident, and it had then already attained a very large size. Probably it had been in process of formation from the date of the paraplegia. The sufferings of the patient were in the end extreme, for he could only make water by placing himself almost on his head, and painful spasms compelled him to do this several times in the hour. The calculus could be felt above the pubes, and it entirely prevented the entrance of a sound into the bladder. At the autopsy it was found to be moulded by the pelvic bones. The ureters were very greatly dilated, so as to constitute "a supplementary bladder," and the kidneys were "altered considerably in their texture, and their pelves much enlarged." Although the notes of this remarkable case are otherwise full, we have no information as to what the state of the urine had been during the formation of the stone. The long duration of life and the slight amount of inflammatory disease in the kidneys, and even in the bladder, would suggest that other sources of the phosphatic salts than chronic cystitis may have existed. No doubt the urine was constantly alkaline, but it seems scarcely possible that it could have been attended through so long a period by severe cystitis. It is moreover

recorded that the patient was accustomed to ride about on a gentle horse. The case, however, affords a good illustration of the enormous size to which in the course of time a phosphatic stone, if left alone, may develop.

A yet more instructive example of the gradual accretion of phosphatic deposit, without any obvious signs of inflammation or even of urinary



decomposition (presumably, for the notes are brief), is afforded by No. 1 in Group G., de-

scribed at page 111 of the Catalogue, and figured in Plate X. In this instance the stone weighed seven ounces and a half, and consisted almost wholly of phosphate of magnesia and ammonia, the quantity of phosphate of lime being very small. It is described in a memorandum from Hunter's pen as "taken from the pelvis of the right kidney of Mrs. —, a natural daughter of Sir Richard Steele. She was never known to have a nephritic symptom till just before her death, when she was taken with a violent pain in her right side near the back, the seat of the right kidney, which appears to have thrown her into a fever, of which she died; and upon opening the body was found this stone filling up an enlarged pelvis, and the substance of the kidney itself become so thin as only to appear like a coat or membrane covering the stone, which gave the idea to the surgeon that the substance of the kidney was grown into a stone." The stone to which this narrative refers is in shape and size much like a kidney. The inner and middle part is of much looser texture than the rest, the latter being compact and laminated. The exterior is smooth. There is no trace of any nucleus of other urinary salt. The composition of this calculus was ascertained to be nearly pure phosphate of magnesia and ammonia,—triple phosphate,—which in the loose textured part was obscurely crystallised.

Dr. Hale White has described, in the 'Pathological Transactions,' a case which is almost an

exact parallel to the above. The stone was taken from the kidney of a dissecting-room subject. It consisted of pure triple phosphate externally, and a mixture of the triple salt and phosphate of lime in the central and middle parts. It weighed 1560 grains. (1835).

A remarkable example of triple phosphate calculi, developing to a very large size in the kidney, has been recorded by Dr. Gee.* Both kidneys were involved, one containing a calculus weighing $36\frac{1}{4}$ ounces, the other one of nearly 10 ounces. In addition to these large ones, there were more than a thousand smaller ones, and much calculous dust. The larger stone had been felt through the abdominal wall during life. The patient was thirty-eight years of age, and had suffered much from inherited gout. He had been liable to pain in the loin from childhood. The stones consisted of nearly pure phosphate of magnesia and ammonia; but in the largest there was a complex nucleus, which contained chiefly oxalate of lime, with traces of triple phosphate, carbonate of lime, and uric acid. The analysis was by Dr. Russell.

This case, in which it will be observed that both kidneys were affected, lends, with the preceding ones, a not inconsiderable support to the belief that the triple phosphate may be secreted in excess and form a primary deposit.

Cases of this kind, so far as the facts go, would

* Medico-Chirur. Trans., vol. lvii. p. 80.

appear to indicate that even triple phosphate calculi may originate in the kidney itself, and grow with but little evidence of inflammation, or of urinary decomposition. It is astonishing how easy it would appear to be, when once the nucleus has been constituted, for the urine to continue to supply material for successive layers. It is a characteristic of almost all the larger phosphatic calculi that they show proof of having been moulded in the cavity in which they have grown. In some instances this results from their mere bulk filling the viscus, but in others it is from the projection of some part of the stone into some adjacent hollow, and its growth by continued accretion in that special direction. There is in the Norwich Museum a magnificent specimen of a branching phosphatic stone developed in the kidney. It is smooth, but otherwise resembles a huge mass of coral.

The discovery of a large triple phosphate stone in the pelvis of the kidney, and without any evidences of inflammatory disease, brings us back to the question as to whether these phosphates are ever secreted in such excess as to be capable of forming concretions. I have already mentioned Prout's statement that he had twice seen the phosphate of lime deposited in large quantities without any evidence of cystitis, and stated that my own experience entirely corroborates his. It is a question needing further investigation as to the precise chemical constitution of the phosphates, which go ordinarily by the

name of "white sand." Usually "white sand" is probably triple phosphate, only in exceptional cases the phosphate of lime. Triple phosphate is, however, a variable salt, and may contain in some cases much less of the ammonia base than in others.

Before leaving the subject of phosphatic urine, we must ask the reader's attention to the extreme facility with which in certain states the urine may become ammoniacal. From the presence of ammonia to the production of phosphatic deposits there is but a step. In connection with this subject some facts given by Dr. Stokes are of interest. That distinguished physician has recorded, in his lectures,* the observation, which he believed to be original, that carbonate of ammonia may be present in the urine quite independently of decomposition or of cystitis. In his first case this happened in a young man who was extremely ill from typhus fever. The ammonia was in sufficient quantity to cause brisk effervescence of the urine, and it continued to be evident for four or five days, disappearing as improvement set in. In order to ascertain whether it resulted from decomposition from retention, the bladder was emptied by a catheter, but the urine taken away two hours afterwards was equally loaded with ammonia. After all, probably this case does not prove more than that when the ferment is

* See New Sydenham Society's edition, vol. ii., p. 311.

present the decomposition of urea may take place with great rapidity. In a second case Dr. Stokes found the urine "loaded with ammonia, and without a trace of urea." In this instance the patient was a strong athletic man, who, after exposure to cold, became seriously ill with pain in the abdomen, tympanitis, and fever. The urine was perfectly limpid when voided, and had not the slightest smell of putrescence. It was alkaline, and effervesced with acids. If the patient completely emptied his bladder and then again in half-an-hour voided a small quantity of water, the latter contained ammonia in the same proportion as before. In this case again, although it may be admitted that the facts are very peculiar, we can scarcely follow the distinguished observer in his inference that ammonia was excreted as such by the kidneys and replaced urea. The very severe pain which the man is described as having suffered may have been due to some one-sided uretral or kidney obstruction, and from this a ferment may have been supplied. As in the former case, all that is really proven is the extreme rapidity with which the whole of the urea in the urine may disappear and be replaced by an ammonia salt. It is to be added, however, that the autopsy showed "the bladder perfectly healthy," and the kidneys only "rather enlarged and turgid with blood."

Those who deny that there is any proof that the phosphates are ever secreted in excess in

connection with long persisting derangements of the general health (the phosphatic diathesis), have to meet the following facts : Undoubtedly there are cases in which for years together, but with variations in quantity, white sand appears abundantly in the urine, there being no indication whatever of disease of bladder or kidney. This white sand consists of mixed phosphates, and is attended by an alkaline reaction of the urine. Next there are in all collections of calculi specimens which consist of white amorphous phosphates, in some cases with a small nucleus of some other substance, but in others with indications that the nucleus also is of the same salt. There are also numerous specimens showing thick layers of dense phosphates intervening between layers of uric acid or of oxalate of lime. The character of phosphatic deposits as thus met with differs from that of the mortar-like encrustations met with as a consequence of cystitis from the presence of an irritating stone, in being much more dense and compact. Not very unfrequently in phosphatic calculi, or layers met with under such circumstances, there is some admixture of urates with the earthy salts. The clinical history of the cases in which white sand appears in the urine is quite consonant with the admixture or alternation referred to. Those who pass white sand often appear to be in good health, or are liable only to dyspepsia, much as are those who pass uric acid, and in some the deposit can be

produced almost at will by indulgence in certain wines or articles of food, just as that of uric acid can in others. There is therefore nothing to excite surprise that the two salts apparently so opposite in their significance should occasionally alternate, being in turn the results of modifications in assimilation not wholly unconnected. Dr. Prout laid possibly too much stress on nervous exhaustion as an accompaniment of his phosphatic diathesis, and hence misled subsequent investigators, who have easily proved that excessive wear of the nervous system is by no means always attended by increase of the excretion of the phosphates. The fact is that the white sand cases are by no means always attended by debility; whilst, as is well known, uric-acid dyspepsia is in its turn occasionally attended by marked debility, and may be produced by overwork and anxiety.*

* Mr. Taylor, an excellent chemist, writes cautiously, "The phosphates *seldom* form the primary deposit." Dr. Henry, writing in 1818, makes the following definite statement:—"In four instances only out of 187, the calculus has been composed throughout of the earthy phosphates; and in these I have not been able to discover a nucleus of any other substance. I consider the fact therefore as sufficiently established that in some instances, though comparatively very few, a tendency to secrete the earthy phosphates in excess is a cause of the formation of stone, first probably in the kidneys, and subsequently in the bladder. This tendency, indeed, as is well known, sometimes manifests itself by the discharge of urinary gravel, consisting of the triple phosphate, either alone or in conjunction with phosphate of lime."

CYSTINE CALCULI.—The cystic oxide or cystine calculus, although very rare, is of extreme clinical interest. It not unfrequently affords us an instance of a urinary disease which prevails as a "family malady." I use the term "family" in its restricted and clinical, not its popular, sense, and as applicable to a disease which, whilst not proved to be hereditary, is apt to occur in several brothers or sisters. Ichthyosis, Kaposi's disease, and retinitis pigmentosa are, with many other structural defects, such as coloboma iridis, our best examples of family diseases. There are, however, many others of less conspicuous character, and amongst these the tendency to pass urine containing cystine or cystic oxide ranges itself. Cystine possesses some of the characters of an organic base, and is soluble both in acids and alkalies. Urine which contains it is of greenish yellow colour, and of a disagreeable smell. It is usually covered by a thin oily film, which consists of cystine mixed with triple phosphate.

The characters of the cystine calculus are well shown in fig. 6, Plate XXIX. It is of a somewhat loose crystalline structure, its crystals radiating from centre to circumference. Its surface somewhat resembles coarse sugar or honey, and it has the peculiarity that on exposure to light it loses its original colour and becomes greenish. Its chief chemical peculiarity is that it contains a large amount of sulphur (19 or even 26 per cent.). Cystic oxide

calculi are remarkable for their purity, thus making it probable that the state of health, or of visceral peculiarity of function, in which they have their origin, is not in any close alliance with the other urinary diatheses.

The College collection contains three specimens of cystine calculi. One, which is the object of our illustration, was taken, after death, from the bladder of a man, aged 21, who had come to St. Bartholomew's Hospital from Northampton, and in whom it had probably been present for a long time. His bladder and kidneys were in a very advanced condition of disease.

In a second case (D. 2) several cystic oxide calculi, the largest the size of a pea, had been voided by an Irish gentleman, aged 40.

The third specimen, D. 3, is part of a large calculus removed by Mr. Luke in the London Hospital from a brewer's labourer of adult age. He had the ordinary symptoms of stone, but suffered no great inconvenience. The stone was extracted with difficulty, and the man died six days later. He had voided calculi fifteen years before his admission, and his urine was habitually turbid from the presence of cystine.

It is unfortunate that in none of the above cases was any enquiry made as to the existence of the disease in brothers or sisters.

Dr. Marcet has recorded the case of two brothers in whose kidneys cystine calculi were found. Lenoir and Civiale each operated on two brothers for the same, and Toel relates the

history of two sisters who both of them voided cystine (quoted by Roberts, page 281).

Roberts also mentions that Mr. Southam had shown him the fragments of a cystine calculus which he had removed by crushing from the bladder of a girl aged 4.

In the Manchester Infirmary is an exception to the general law that cystine calculi contain no other ingredient, for the stone consists of a uric acid nucleus, a body of pure cystine, and an external layer of mixed cystine and uric acid; whilst finally there is a phosphatic encrustation. Cystic calculi appear to be usually formed in youth, and to be very apt to cause irritation and disease of the bladder.

St. Thomas's Hospital Museum contains two specimens of cystine calculi. Respecting one (D. 2) no details whatever are given. The other (D. 2) is of special interest, because the cystine alternates, and is mixed with the oxalate of lime. The nucleus is of cystine, and there are also "green rings" of the same.

Guy's Museum contains several specimens of cystine calculi, and amongst them the original one described by Dr. Marcet, who found it by accident in the collection. In none of these is there any statement as to family prevalence. In most the tendency appears to have begun in early life, and to have persisted for long. One of them was removed by Mr. Key from a young gentleman, aged 12, whose urine also contained cystine. We have here an exception

(proving the rule) that the children of the upper classes never come under care for stone in the bladder.

In St. George's Hospital Museum is a cystine calculus removed by Mr. Cæsar Hawkins from a Cornish boy, aged 6, who had suffered for four years. The central part only consisted of cystine. More externally are layers of urate of ammonia and oxalate of lime.

Mr. Taylor, in 1838, described two cystic oxide stones in the St. Bartholomew's Collection, one of which he believed "the largest and most perfect in the kingdom." It was supposed to have been taken from the bladder of a young man after death, in whom during life there had been no symptoms of any affection of the urinary organs. Unfortunately no record of the case exists. The stone weighed 740 grains; the other, of which the museum possesses only the half, weighed 155 grains.

Mr. Christopher Heath has published the case of a man, aged 28, from whose bladder he removed a number of cystine calculi. Other concretions were subsequently formed, and death from cystitis, &c., occurred two years after the lithotomy. A small quantity of cystine had been invariably present in the urine.

Dr. Ord exhibited, at the Pathological Society in 1879, a cystine calculus, probably the largest on record, which weighed 1050 grains. It had been successfully removed by lateral lithotomy from the bladder of a young man under the

care of Mr. Reginald Harrison, of the Liverpool Infirmary.*

Mr. Shattock showed, at the Pathological Society in 1880, for Mr. Steele, of Hemel Hempstead, a calculus in which cystine, with a minute admixture of oxalate of lime, composed the nucleus and chief bulk. There was, however, a thin layer of dark-coloured oxalate of lime, over which again cystine occurred. It had been removed from a healthy lad of 19, who since the age of 4 had repeatedly voided calculi. A great-grandfather had died of stone, and a sister had obscure renal symptoms. Two years after the lithotomy there was no recurrence, and, as in the preceding case, no cystine could be found in the urine.

In the Medical Museum, Bombay, is a cystine calculus removed from a boy of four (a Parsee). Cystine was also detected in the urine.

I do not find that any observers have paid special attention to the nuclei of cystine stones. It has been assumed that it is usually cystine. In the one of which we give a sketch, and in several others referred to, it appears to be not so.

ON ALTERNATION OF LAYERS.—In explanation of the alternation of layers of different salts, there can be little doubt that we ought to call to our aid in many instances the influence of drugs,

* In 1882 Mr. Harrison reported that there had been no recurrence, and, what is very singular, that cystine had never been detected in the urine.

and of abrupt alterations of dietetic habits in obedience to medical advice. A large majority of our stone patients have at one period or other been under medical care: it may have been for the dyspepsia which attends lithiasis, or it may have been for symptoms directly referable to the kidneys or bladder. When we see in the section of a calculus an abrupt demarcation between a layer of lithic acid and one of oxalate of lime—it may even be between earthy phosphates and one of these—we may suppose with plausibility that they denote either change in season and diet, or in medical treatment. It is not likely that without some such influence the patient's diathesis or state of health would so suddenly alter. Uric acid may be taken to represent in the main the colder season and the heavier diet; the layer of oxalate upon it may often denote the summer season with its rhubarb and fruit at meals; or, without change of season, the oxalate layer may have been caused by the patient's having sought advice for lithiasis, and been ordered to at once give up beer, take less meat, and use fruit freely. In this way lithiasis may easily and suddenly have been made to give place to oxalaria. When phosphatic layers are seen in alternating calculi,—and they are not so uncommon as has been supposed,—they may possibly mark the time when the patient, alarmed at the bladder irritation which he suffered, not only changed his diet, but took to the free use of alkalies. When the latter had weakened him

and brought out boils, then he gave them up and went back to his better food, with the result that his urates or oxalates were again thrown down. In some instances an abrupt and only temporary change in the deposit may represent some period of acute illness. I give these simply as plausible suggestions. Of the general fact there can be no doubt; we may read, if we can, in the section of a calculus, a record written in stone of the sufferer's changing habits and varying states of health. In this possibility consists one of the chief interests of our topic.

The term "alternating calculus" is applicable to any stone in which layers of different material recur repeatedly. Good examples of alternation, that is with several repetitions of the different salts, are not very common; but alternation to some extent is exhibited very frequently indeed. The salts which alternate are usually lithic acid and lithates with the oxalate of lime, and these salts often afford very conspicuous differences in colour. We may note in passing a deceptive appearance of alternation is sometimes produced by differences in colour in different layers of the same salt.

As a rule the phosphates are not found in the substance of alternating calculi. This observation was first made by Dr. Marcet. When the phosphates occur in large quantities they are usually evidence of cystitis and of decomposition of urine, and after this has once occurred the

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deposition of pure uric acid, or of oxalate of lime, is, for the most part, impossible. Since cystitis caused by a calculus is very rarely cured until the stone is removed, it follows that when once the encrustation of phosphates has begun it will continue to the end. Hence the explanation of Dr. Marcet's law, and not, as it was formerly the custom to say, that the phosphatic diathesis had superseded that of lithiasis.

Respecting alternating calculi, Mr. Taylor observes, "that in no one instance (in the St. Bartholomew's collection) have the phosphates, either pure or in a state of mixture, formed the nucleus." He quotes the highest authority on the subject (Dr. Marcet), to the effect "that a decided deposition of the mixed phosphates is not followed by any other deposition." He then describes a stone which appears to be, he says, an exception, but suggests as highly probable that the deposition of phosphates was caused by the use of alkaline remedies. The stone in question has a nucleus of urate of ammonia, then oxalate of lime, then a white layer three-eighths of an inch in thickness; outside this white layer, which consists of mixed phosphates, is a thin layer of oxalate of lime, and next crystalline uric acid, ending with mixed uric acid and oxalates.

Exceptions to Marcet's law are, however, not very infrequent, and our plates illustrate several examples of them. Thus in Plate XXXI., figs. 1, 3, 11, 12, and 14 all show the appearances

of deposits of phosphates which have been subsequently covered in by oxalate of lime or lithic acid. In Plate XIX., fig. 9, is again a beautiful example of the same, but it must be understood that the conditions displayed in these stones is very exceptional. Good examples of alternation of uric acid and its salts with oxalate of lime, and of the encrustation of both by phosphates, are seen in so many of the sections that it is scarcely worth while to specialise them. The phenomena of alternation give support to the conclusion that the states of health which permit the deposit of uric acid and of the urates and oxalate of lime, are very closely allied. In the section of an alternating stone we may, as has been remarked already, in some sense read the record of the life of the patient in whose bladder it was formed; a slight alteration in his health, or very possibly in his diet, had clearly caused a deposit at one time of uric acid, and at another of the oxalate; it may be that this change has chiefly to do with the season of the year, and that during the summer, when vegetables and fruit were plentiful, the oxalate took for a time the place of the uric acid which was the more ordinary deposit. In children the reverse of this may be the case, and in connection with their peculiarities of diet, oxalate of lime may be the usual deposit and uric acid the exceptional one.

Of singular interest in connection with this subject is the calculus which is numbered 12 in

Plate XXXI. In this stone variety of alternation is beautifully shown, and amongst the salts which compose its layers is the phosphate of lime, not once only, but twice, in the most definite manner. The stone begins in its centre by urate of ammonia; around this is oxalate of lime, and next nearly pure uric acid; then comes a thin white band of earthy phosphates, then uric acid again, next a much thicker layer of phosphate, and lastly an encrusting layer of oxalate. The limitation between these different layers is as abrupt as possible, and it is certainly most difficult to believe that on two occasions cystitis was present for a short time, and then suddenly ceased and gave place to a state in which the urine was acid in excess. The fact that the phosphate layers do not contain the triple salt is also strongly in opposition to the suggestion of cystitis as its cause. That something of the nature of a diathesis prevailed at the time of deposition of the white layers seems far more probable.* Such diathesis may, it is true, very possibly have been to some extent

* Amongst the extraordinary discrepancies of opinion on these subjects it must be noted that Dr. Owen Rees denies categorically that the use of alkalies has any tendency to favour the deposit of phosphates (page 302). He was perhaps the first to doubt the existence of a phosphatic diathesis. The only alkalinity of the urine which he allows to be capable of causing phosphatic deposit is that caused in the bladder by cystitis. Upon this theory he based the practice of giving alkalies in phosphatic urine in order to make the urine less irritating, and so prevent alkaline secretion from the bladder.

artificial, that is, it may have been helped or even caused by the remedies which were being used. The phosphatic layers may possibly represent periods during which the patient's urine was kept alkaline by the use of drugs. On the other hand, they may have been due to changed conditions in the general health in connection with diet, climate, or season. Fig. 1 of the same plate and fig. 9 in Plate XXIX. show almost the same conditions almost as well, and a repetition of them is admirably exhibited in fig. 10 of Plate IX. of the College Catalogue (not copied here). To a slighter extent they are seen also in fig. 3 of Plate XXXI., where a narrow, white line of phosphate surrounding an almost black oxalate stone is in its turn covered, first by oxalates, and next by uric acid.* In seeking an interpretation of such appearances we must never forget the possible influence of vigorous drug treatment. To make the urine alkaline, and to keep it so for a certain time, would probably be quite sufficient to permit the encrustation of a calculus with earthy phosphate, whilst the tendency to deposit of this salt would cease at once when the treatment was changed.†

* The oxalate of lime often follows the urates, but the urates rarely follow the oxalates.—Dr. Prout, Dr. V. Carter, and others.

† Mr. Bransby Cooper, in the Guy's Reports for 1849, has figured some beautiful examples of alternating calculi. In one of these (fig. 4) as many as fifteen or sixteen different layers may be counted; and figures 3 and 5, the latter from the Norwich Museum, are almost as good. Figures 4 and 5 both show phosphates alternating with lithates and lithic acid.

The museum of St. Thomas's Hospital appears to contain many specimens which afford exceptions to Marcet's law, or at any rate illustrate the simultaneous deposit of uric acid and of fusible phosphates. A majority of the specimens in the fusible group are described as having uric acid mixed with the phosphatic salts. The same statement applies to the concretions around foreign bodies. In this feature the results of analysis differ widely in the St. Thomas's series from those recorded at the College of Surgeons.

MIXED CALCULI.—The term "mixed" has been given to stones in which the different ingredients are mixed indiscriminately together. Observers agree that it is most unusual to find stones which, as a whole, deserve this appellation, and it is not in the least difficult, as a rule, to place a stone in its proper group as consisting of one or other salt, or as alternating. Slight amounts of mixture in certain layers are, however, common, the most frequent being of the urates with uric acid, or with phosphates.

ON THE NUCLEI OF STONES.—Much attention has naturally been given to the ascertainment of the precise character of the nucleus in calculi. The nucleus is clearly of extreme importance in reference to their formation, for a patient may go on for years passing either red or white sand in great profusion, and yet have

no stone form. The moment, however, that a nucleus has been produced, then the process of accretion will be established, and the salts will be retained in the bladder, and will add to the bulk of the growing stone.* So far as is at present established, however, the formation of the nucleus is much a matter of accident, and the latter will usually consist of the same salt, or a near ally to it, as that which constitutes the bulk of the stone,—in other words, of that substance which most usually is in excess in the patient's urine. That the addition of animal matter is commonly necessary, and that the process is something more than merely one of either deposition or crystallisation, is highly probable.

* Dr. Vandyke Carter has devoted much attention to the microscopic structure of calculi and their nuclei. One of his chief conclusions is that the crystalloids are found in spheroidal forms, or in forms tending to the spheroidal, in a colloid bed. He suggests that "an excess of mucus, perhaps altered in character, in the urinary passages, or the effusion of albumen, fibrine, or blood and the like, ... would furnish a colloid medium, with which uric acid, the urates or oxalates,—themselves, perhaps, in excess,—would combine." Mucus is for Dr. Carter the most important colloid in the formation of calculi. It is now ascertained that very frequently does the commencement of a stone seem to be due to accumulation of normal formation of the globular urates; that not seldom is the oxalate in submorphous form the first material laid down, and that comparatively seldom do crystals of uric acid compose the nucleus of a calculus (Vandyke Carter, page 29). At another part of his Essay, however, Dr. Carter states that "unmixed with oxalates the urates have never been found in calculous matter."

Dr. Carter, Dr. Ord,* and others have urged that a peculiar colloid material is the ordinary cement.

By Dr. V. Carter an attempt has been made to show that in India, at any rate, the nucleus is usually oxalate of lime, and his conclusions have received some support from others. It is certain, however, that this is by no means constant, but that the oxalate, uric acid, urates, phosphates, mucus, and even blood, may in turn constitute the centre of accretion. We shall probably be not far wrong in believing, as above hinted, that the nucleus is usually formed of the salt most often in excess, and that it is formed during some temporary period of unusual concentration of the urine,—with, it may be, an attack of renal congestion and increased mucous secretion.

In Bristol, oxalate of lime as a nucleus is stated to occur in 40 per cent., a yet larger proportion than in Bombay, whilst Mr. Bransby Cooper, in

* The experiments of Dr. Ord, based, as he states, on suggestions by Mr. Rainey, bear out the conclusions previously arrived at by Dr. Carter as to the importance of a colloid (mucus) in determining the concretions of uric acid, &c. As he forcibly puts it, the pebbles of the concrete would not hold together without the cement to bind them, and to act on their surface. To make calculi of uric acid without colloids would be as hopeless a task as making ropes of sea-sand." — 'Medico-Chirurgical Transactions,' 1876. We must not, however, forget that the irritation caused by urinary sand of various kinds is probably in most cases itself the cause of the formation of the mucus which acts the part of the cement.

thirteen calculi from the Punjaub, found urates as the nucleus in all but four :—

8	Urate of ammonia.
3	Uric acid.
1	Oxalate of lime.
1	Phosphates and urates.
13	

Dr. Carter confirms Mr. Cooper thus far, that he admits that in most alternating calculi urates precede oxalates.

Dr. Beale long ago experimented upon lithic-acid calculi by dissolving them in potash, and convinced himself that a nucleus of dumb-bell and spherical crystals of oxalate of lime was usually left behind. He found concretions of the same kind in the pelvis of the kidney, and even in the uriniferous tubes themselves, and believes that the salt named supplies the true nucleus in almost all calculi. He figures microscopic concretions of it as voided in the urine, and also as occurring in the kidney-tubes. The careful examination of uric-acid gravel, which is common, would easily decide this point.

Blood, as a rule, does not irritate the bladder, and its coagula are usually disintegrated and pass away. It is only when a pre-existing state of the urine favours deposition that we find fibrine constituting the nucleus of a calculus.

One of the best examples of the fibrinous or blood-nucleus on record is a case published by Mr. Bransby Cooper in the Guy's Hospital Reports. It is one in which the whole history

is complete, and its lesson is emphasised by the fact that there were two calculi, and they were exactly alike. Mr. Cooper removed the two by lithotomy from the bladder of a boy. They were about the size of small greengages, and consisted of layers of uric acid and urates. In the middle of each was a cavity capable of containing two peas. In one the cavity was quite empty, but in the other it contained the remains of fibrine. The history was that two years before the operation the lad (a plasterer) had fallen from a scaffold and contused his loins. After the accident he passed blood, and had great difficulty in micturition. He was soon well from the injury, but symptoms of calculus in the bladder followed.

The only deficiency in this narrative is that we have no statement as to the condition of the boy's urine before the accident, or as to family tendencies in this respect. In all probability he was the subject of lithiasis, or had a strong hereditary tendency to it. Mr. Cooper is careful to direct attention to this probability, but misses the opportunity of recording the fact. He well writes:—"Although in almost every case in which a hard foreign substance forms the nucleus of a calculus, the deposit consists of the earthy phosphates: in some instances a foreign substance may lead to the deposition of uric acid or urate of ammonia. The nature of the matter thrown down from the urine under any circumstances must, however, depend upon

the state of the constitution, and upon the condition of the urine itself."

The following is Dr. Golding Bird's summary as to composition of the nucleus in the calculi in the Guy's Hospital Museum at the date of his paper:—

Nucleus consisting of Uric Acid or Urates	262
„ Cystic Oxide . .	11
„ Oxalate of Lime . .	45
„ Phosphates . .	21
	339
Mixed Calculi . .	3
	342

A fallacy which appears to have puzzled Dr. Prout (as it well might) in the above statements concerns the large proportion of cystine nuclei. The author forgets to tell us that the eleven were all from one patient. This information I get from another source. It may be that similar errors underlie other statistical statements on this subject, and explain seeming discrepancies.

In seeking to determine the starting-point of a calculous concretion the operator must take great care that he does not miss the true nucleus. It is very possible that the line of section may not pass exactly through the centre of the stone, and that it may exhibit what we may describe as the kernel rather than the true nucleus. It is very common for a change in the nature of the salt deposited to occur very promptly after the formation of the initial

crystalloid nucleus.* The latter may be very small, and its presence in the centre of a larger and much more obvious kernel may easily be overlooked, or, as just suggested, it may not have been disclosed in the section. It might possibly be convenient, in describing stones, to use the term "kernel" in a looser sense than that of nucleus, and as applicable to the more conspicuous middle concretion, restricting the latter term for the carefully ascertained centre of this. In this use of the words it would probably be found that many published state-

* In using, for statistical purposes, the statements recorded by surgeons as to the composition of calculi and their nuclei, some margin—and, perhaps, not an inconsiderable one—must be made for possible errors. Operators are, I believe, usually content to classify their stones by the naked-eye appearances, without obtaining the aid either of the chemist or microscopist. The fallacies here are very great indeed; nor are they wholly removed even when a chemical examination has been made. Mr. Wagstaffe has published a case which well illustrates this. He showed, at the Pathological Society, some concretions from the kidney which he and others, after using chemical tests, believed to be calcium carbonate. Subsequently, having heard that another analyst had said that they were calcium oxalate, he requested the Chemical Committee of the Society to give its verdict. This was to the effect that one part was calcium oxalate, but that the greater portion was calcium phosphate, with only a trace of the carbonate. Not the least important point in this narrative is that one of the calculi was "of a dark brown colour, very hard, and closely resembled uric acid in appearance." Yet all the observers agreed that no trace of uric acid could be found, however much they differed on other points.

ments have reference to the kernel only, and not the true nucleus.

It may not be out of place to remark that the study of nuclei may be very profitably made upon gravel (happily out of reach of destruction by the lithotritist). In cases of multiple calculi, and of gravel, the operator has an opportunity for the examination of the conditions in many specimens formed under the same circumstances, and the risk of error in his conclusions is much diminished. When the calculus is single, it is obviously more probable that its beginning may have been, so to speak, a matter of accident than when multiple. It is in the multiple cases that we must seek to find illustrations of ordinary laws.

When calculi are hollow, as is now and then observed, the explanation usually given is that



a considerable clot of blood has formed the nucleus, which has subsequently been absorbed.

The College Museum contains some extraordinary specimens in which saponaceous matter in large masses forms the centre of the stone, the supposed explanation being that soap had been injected into the bladder. The appended woodcut is a representation of one of these.

Calculi containing Indigo.—Dr. Ord has described a calculus removed from the kidney of a woman who had died from sarcoma which had obstructed the ureter of the opposite side, in which the concretion consisted chiefly of a substance resembling indigo. It is well known that under various conditions a blue colouring-matter like indigo may be met with in the urine. It may occur, for instance, in carbolic-acid poisoning. Dr. Ord thinks that it may be produced whenever there is any unusual or rapid breaking-down of albuminous matter. (See ‘Pathological Transactions,’ Vol. XXIX.).

On the Influence of Sex in relation to Stone.—The very remarkable difference in the numbers of the two sexes who suffer from stone has been hastily supposed to be satisfactorily explained by the greater length of the male urethra. This may be influential to some slight extent in preventing the escape of gravel, but it is not likely that it is very important. If once a stone has entered the urethra from the bladder it is not very likely to slip back again, and the length of the male urethra, its curves, &c., whilst they would well explain the occurrence of impaction, would scarcely account for retention of gravel in

the bladder. It may, I think, be established that not only is it very rare for women or girls to pass gravel or small stones, but that they suffer but seldom from either uretral colic or lithiasis. Careful observations on this point are desirable, but my own experience has been that whilst I have repeatedly had boys brought to me in whom the urine was habitually loaded with uric acid, exceedingly few such cases have ever come under my notice in girls. I cannot extend this statement to adult women, but I can scarcely call to mind a single example of a definite attack of uretral colic in a female. It cannot, I think, be doubted that quite apart from the supposed greater facilities for escape, the tendency to the formation of calculi is in women at all ages far less than it is in males. To some extent we may credit differences in diet with this, but it must be borne in mind that it is true of young children quite as definitely as of adults. Lithiasis is a malady to which, in virtue of inherited organisation, males are preferentially prone.

In making these remarks we must not forget that it is possible that in the male sex in all ages the bladder is frequently less completely emptied than in the female. In the latter the stream of urine is probably, as a rule, much larger than in males, the time taken in micturition much shorter, and the evacuation of the bladder usually more complete. It is possible that many boys and men never absolutely empty

the bladder. If this be so it is obvious that not only is the deposition of the salts favoured, but the escape of gravel, which frequently takes place with the last drops, is hindered.

Amongst the many facts in reference to calculous disorder, which it is extremely difficult to explain, it must be mentioned that all surgeons of experience amongst the richer classes agree that it is most rare to meet with stone in children belonging to them. Amongst the poor and labouring classes boys often suffer, and it appears to be the case in all parts of the world that about two-thirds of the total of stone operations are done in boys. Amongst the wealthier classes lithiasis in boys is common and troublesome, and cases of gout not very uncommon; but for some inexplicable reason vesical calculi form with extreme rarity. They are not absolutely unknown, but their frequency is not at all that which might have been expected.

Boyhood and the early stages of old age are the periods in life in which stone in the bladder is most likely to occur. This appears to be equally the rule in all parts of the world in which calculus is common. When stone occurs in boyhood its early stages are often very insidious; indeed it is by no means improbable that most calculi at this age have their beginning in the earliest period of infancy. The blocking of the renal tubes with salts is a phenomenon not unfrequently seen in dead-born infants, and that nuclei of stones may

be thus formed is easily understood. It can, however, by no means be supposed that all calculi in boys originate at this early age, and indeed the occurrence of lithiasis, so conspicuous as to attract attention, is by no means uncommon in boys. Yet attacks of uretral colic are very rare, and so, too, are renal calculi. As age advances the occurrence of symptoms indicating the passage of a stone down the kidney becomes not infrequent, or it may happen that recurring attacks of pain, with occasional appearance of blood in the urine, indicate that a stone remains fixed in the kidney. Why the adolescent and early manhood periods of life should be, like the female sex, comparatively exempt from stone, it is very difficult to say, but the fact is universally observed that if a young man comes under care for stone his case is probably one which has been long neglected. At such ages the stones often prove to be large.

On the possible explanations of local prevalence of Calculus.—That there is such a peculiarity—we must scarcely call it a disease—as polydipsia (not alcoholic) is well known, and also that the affection is hereditary, and may prevail in families. We have it, I think on the grave authority of Dr. Gee, that an infant of a family in which this tendency was known to exist began to cry for water as soon as it was born, and did not cease until allowed to freely appease its thirst. Now in such persons the formation of calculi would probably be impossible, and it

is highly probable that careful observation would show that the tendency to polydipsia of this kind in minor degrees is far from uncommon. Indeed, every mother knows that children differ very much as regards thirst. If peculiarities in this respect could be shown to prevail in races or communities we might possibly have one clue to the explanation of the local prevalence of stone. It is possible also that in districts where the water is agreeable to the taste the general tendency to drink freely of it may be much greater than in districts where it is disagreeable. As an agent in favouring metabolism and elimination, as well as in keeping the products of waste in perfect urinary solution, the free consumption of pure water is doubtless of very great importance. In districts where stone is common this matter should receive careful attention. The prevalence of stone in Syria, Arabia, and Persia, and many mountainous districts in hot countries where the supply of water is probably limited, may perhaps be explained in this way. Throughout the United States, where calculus is very rare, although a large part of the population may be supposed to have some inherited proclivity to it, the supply of drinking-water is, I believe, everywhere good and liberal, and its customary consumption far greater than in England. Facilities for drinking are systematically provided on a scale of which we as yet know nothing, although amongst ourselves great

advances have been made during the last quarter of a century.

Mr. Copeland Hutchison published, in 1818, in the 'Medico-Chirurgical Transactions,' a paper in which he cited much evidence in proof of the infrequency of stone in sailors.* His conclusions have, I believe, been sustained by subsequent experience, and the problem remains, why should it be so? Seafaring persons use salt provisions, and but little of fresh vegetables; they take much exercise and sleep in very close, ill-ventilated quarters. They drink but little beer, and, it must be added, they are liable to great variations in diet, often taking during short periods on shore an excess of those articles of which they have been deprived whilst at sea. That it is the comparative abstinence from fresh vegetable food seems improbable when we remember the frequency of stone in India. That the influence of exposure to air and wind is in some way favourable to completeness of assimilation is perhaps the most probable hypothesis. Sailors are said to be great consumers of salt when on shore, but it may be thought difficult to see why salt meat, or fresh salt with meat, should be preventive of lithic deposits in the urine. Here, however, an important suggestion has to be made.

* Dr. Hutchison calculated that in naval hospitals only one in ten thousand patients was a case of stone, whilst Dr. Marcet had estimated that in the general hospitals of England and the Continent, Norwich excluded, the proportion was one in three or four hundred, and in Norwich itself not less than one in thirty-eight.

Dr. Bence Jones, in an important communication to the Medico-Chirurgical Society in November, 1843, showed that the urate of ammonia is made much more soluble in urine by the addition of a moderate quantity of sodium chloride. Bearing in mind Prout's opinion (sustained by Bence Jones) that uric acid probably in the first instance exists in the urine as urate of ammonia, we may have here a very important clue to the explanation of the formation of calculi. It may be that the tendency to calculous formations is much greater in those who take but little salt. Some persons, as a matter of individual taste, take very little salt, and some take it very freely. These tastes are transmitted hereditarily, and may become prevalent in certain families or even races. In some communities and districts the consumption of salt may be much reduced either by custom or by difficulties in obtaining it. On the other hand, under some conditions of life (sailors, for instance), the free consumption of salt may be more or less compulsory.* Another point in

* In a short paper by Dr. Max Gruber, of Graz, on the 'Influence of Common Salt on the Reaction of Urine,' the author details the results of some experiments on dogs. The following are his principal conclusions:—

A meal rich in common salt, given to a dog after several days of a nearly salt-free diet, makes the urine intensely alkaline in a few hours. (1) The effect is proportionate to the dose. (2) Even after small doses the effect is noticeable. (3) The urine becomes alkaline after the injection of salt solution into the stomach, or into a vein. (4) The change is only noticeable for the first, or

reference to the free use of salt is that it increases thirst, and thus leads to the drinking of large quantities of water, the solvent action of which may in its turn further reduce the tendency to calculous formation.*

at most the first three days of the salt-diet, after which it regains its normal condition. (5) If a saltless diet be persisted in, the urine no longer becomes alkaline during digestion. (6) Hydrochloric acid is retained in the stomach and sodium carbonate eliminated.

The Essay from which these experiments are quoted is one of a series presented to the venerable Prof. Ludwig on his seventieth birthday by his pupils, and written expressly for that object.—‘*Beiträge zur Physiologie*,’ Leipzig, 1887.

* I had written all that occurs in the text above and had become deeply interested in the salt question before I knew that the whole subject had already been worked out. On a visit to Mr. Cadge, in Norwich, in May last, in search after facts as to calculus in Norfolk, I was made acquainted with a paper by Dr. Plowright, of King’s Lynn, which I had previously overlooked, and with that gentleman’s belief that the use of salt in food is an important factor in respect to the prevalence of stone. I have since read Dr. Plowright’s very able papers, which contain a summary of much earnest work in search after the truth in this important matter, and I have had conversations with their author. He has investigated the statistics of stone in Norfolk in relation to the amount of chloride of sodium in the drinking water, and also the history of the frequency of stone with reference to the salt tax. I shall not attempt here to re-produce his detailed statistical statements, and no one would, I feel sure, be more ready than himself to admit that numerous possible fallacies are involved in them. Making, however, all allowance for risk of error, it is not easy to escape the impression that the facts do bear strongly in support of the belief that the free use of salt is a very valuable means of preventing urinary concretions. It acts not only as a base for lithic acid,

There are probably many other questions of detail in reference to diet which it is desirable to

but as a solvent for the colloids which take such an important share in the formation of the nucleus. In justice to Dr. Plowright's researches I append his conclusions in his own words. His paper is to be found in the 'Medical Times and Gazette,' Oct. 10, 1885:—

"The conclusions I would derive from the foregoing are:—(1) That there exists in Norfolk a district (Marshland) in which calculus is much less common than in other parts of the country. (2) That in Marshland the natural water-supply contains a large proportion of salt. (3) That calculus was relatively more common during the period of the salt tax than before or after. (4) That the disease is rare amongst sailors who consume much salt. (5) That it is more common amongst the children of the poor from whose dietary salt is almost absent, than amongst those of the middle and upper classes, where this is not the case. (6) That the disease is common in India, where salt is taxed. (7) That the presence of salt greatly increases the solubility of uric acid. (8) That the consumption of salt by increasing thirst ensures a larger amount of fluid passing through the urinary tract, and therefore lessens the probability of calculus. (9) That by keeping the colloids equally diffused, salt tends to prevent the crystalline solids of the urine from agglomerating into calculi. And lastly, that the facts above stated warrant us in recommending a more liberal consumption of salt by those who are in any way threatened with calculous disease, either by hereditary tendency or by premonitory symptoms—especially those at the two extremities of life who reside in calculous districts. Particularly does this apply to infants and young children—the former, of course, through the mother, for we know that sodium chloride is one of those salts which freely passes from the maternal system into the mammary secretion." Dr. Plowright has followed up the subject in more recent papers; see 'British Medical Journal,' Nov. 7, 1885, and a pamphlet on the 'Cause and Distribution of Calculous Disease,' 1886.

keep in mind whilst investigating the causes of gravel and stone. I have already alluded to the possible influence of fruit in causing oxalates, and have also said that some patients experience a deposit of phosphates after taking certain wines. It is possible that other articles of food may have like results.

In his work on urinary diseases, page 286, under the head of phosphatic deposits, Dr. William Carter,* of Liverpool, states that "patients are frequently much concerned at observing a turbidity in the urine when first passed. I have known such urine to have been passed for many weeks together. The turbidity seldom affects that which is passed the first thing in the morning. But the urine at other times of the day is alkaline or neutral, coated often by a greasy film, and throws down a dense deposit of usually amorphous phosphates." He describes the turbidity as greatest at the end of micturition, and says that the malady is usually cured by care in diet and the use of pepsine with nitromuriatic acid. He cites two cases (page 82) in

* Dr. Carter's cases appear to be examples of what, when in a severe form, is known as phosphatic diabetes, or, more properly, polyuria without sugar and with excess of phosphates (sugar may be found later on in the case). This condition corresponds with Prout's azoturia. Dr. Dickenson was the first to point out that in certain cases of polyuria there was a great excess of the earthy phosphates, and to connect it with tissue-waste. In a paper by Dr. Ralfe two cases are mentioned, both in delicate young men. ('Pathological Transactions,' Vol. XXIX., p. 152).

which this condition of urine was caused by taking oatmeal. In one of these a medical friend was inconvenienced by very frequent micturition. The urine was copious, acid when passed, but rapidly decomposed with abundant deposit of phosphates. Dr. Carter directed his friend to leave off oatmeal porridge; and with great advantage. When it was taken again after an interval the same symptoms followed. In a case in which the free use of oatmeal was prescribed experimentally similar results were produced. He believes that the too free use of "scones" is also liable to suspicion in the same direction.

Many of the Indian uric-acid calculi are very deeply coloured, and some of them of a peculiar tint of red, not often seen in English stones. A collection in the College of Surgeons' Museum, presented by Mr. Holmsted, illustrates this remark. One of them is very dense, quite smooth in section, and of a purplish-red colour. Mr. Bransby Cooper mentions, amongst those which he examined, "one of a peculiarly red-tinted uric acid mixed with urate of ammonia." This fact is not without its interest in connection with Dr. Ord's experiments, which seem to show that colouring-matter exercises an important influence in determining the sedimentary urinary form of uric acid.*

As an interesting illustration of how completely one generation sometimes rids itself of

* 'Medico-Chirurgical Transactions,' 1876.

the prejudices of a predecessor, we may note that a dietist of the present day is found timidly apologising for allowing lithiasis patients any meat whatever; whilst little more than fifty years ago Dr. Marcet wrote:—"It may be worth while to observe that however useful animal food may be in counteracting the generation of acid, it would probably not be found advisable to restrict calculus patients to the exclusive use of animal food with a view to produce this effect." He adds farther on:—"It might be found detrimental to restrain patients affected with this kind of calculus (lithic acid) from taking a due proportion of vegetable nourishment." In Dr. Marcet's day vegetables were supposed to be the source of the acidity or "acescent tendency," which resulted in calculus. This view of the matter is worth keeping in mind in reference to the remarkable and unexplained frequency of calculus among the natives of India.

The Distribution of Calculus in reference to Climate, Race, &c.—The distribution and prevalence of calculous disorders, in reference to climate, race, and position in life, offer most attractive subjects for investigation. It might at first sight be supposed to be comparatively easy, since the diagnosis of stone and gravel is not difficult, and examples of their occurrence attract the attention of all concerned, being readily remembered and usually accurately registered. There are, however, as yet certain special difficulties which prevent our feeling

sure that the facts as we now possess them are quite trustworthy. Thus, respecting many countries and races, we have only the impressions received by medical travellers who resided for only short periods in the places concerning which they report. Next comes the fact that the statistics of cities, if these be placed in large districts not well supplied with operating surgeons, may, by the attractions of hospitals, represent a seeming prevalence of stone far greater than is really the case as regards the whole population. In other words, we can never accurately estimate the population from which any given institution, with the statistics of which we are dealing, draws its patients, and until we can do this it is impossible, without much risk of error, to state the percentage of those who suffer from stone. It might at first sight be thought that the mortality statistics would help us, but here occurs the overwhelming fallacy that stone is a curable disease, and that wherever it is prevalent there usually skill in its treatment is developed, and the deaths become fewer. There can be little doubt that the mortality from stone in adults has much diminished of late years, and that it will diminish still more in the future; but this will not prove any reduction in its prevalence. It must be noted also, as a possible source of error in our impressions, that in some instances in which statistics have been collected they have been formed on the deaths from stone, and in others

upon the cases presenting themselves for operation. If, however, we carefully keep in mind, in our enquiries, these various sources of error, and make due allowance for them, we may venture to trust the general bearing of the facts.

For the purpose of this enquiry we may count all calculi as belonging to the uric-acid group, with the single exception of the phosphatic stones. Uric oxide is only a variety of the uric acid, and besides, both it and xanthine are so rare as not in any way to affect statistics. Oxalate of lime may be reckoned as a modification of uric acid, and as depending upon the same diathesis. The two always go together as to frequency, and are, indeed, often mixed in the same concretions. Respecting the phosphates the case is different, for, if we make allowance for a small number which may possibly be primary and result from diathesis, they always imply some disease of the urinary organs, and must be expected to occur wherever such affections as paraplegia, stricture, enlarged prostate, chronic cystitis, and vesical fistulæ are met with. In districts where calculus is reported to be almost unknown it is probable that the exceptions will concern phosphatic concretions. These we must expect to meet with now and then everywhere, and in all races.

Our enquiry, then, concerns the local (or geographical) and race prevalence of the lithic-acid diathesis as evidenced by the occurrence of calculus in the bladder. In the first place let it

be stated that if we seek for the causes of varying prevalence of this diathesis, we have before us a problem which has not as yet received its solution, and which, indeed, in spite of the great labour already devoted to it, seems almost as far from being understood as ever. We have stone so exceedingly common at the Norwich Hospital that one or more operations are performed every month, whilst at Belfast it is so rare that a student may complete his curriculum without having ever seen a single case.* We have it unknown, or almost so, in the African races,† whilst it is so common in India that it has been asserted that “walls might be built or garden paths covered” with stones obtained from the human bladder. Again, we learn that it is very common in and around Moscow, but exceedingly rare in other parts of Russia, a precisely similar statement being true of the Canton district and the rest of China. Such facts are exceedingly difficult to explain, excepting on the *prima facie* hypothesis that calculus must depend upon peculiarity in the geological formation of the district and the water-supply which it yields. It seems impossible that diet, climate,

* In reference to the frequency or otherwise of stone and lithiasis in Dublin, I may note that Dr. Stokes, in his admirable lectures, which embrace such a wide variety of clinical topics, does not mention either lithic-acid calculus or gravel. He has, however, a good deal to say about gout.

† No. 2214, 33, in the Guy's Hospital collection, is a urate and oxalate stone from a negro boy at the Cape.

or race can have much to do with the question. But this plausible conjecture as to water-supply, although at one time warmly espoused by some investigators, breaks down entirely when it is critically examined. In the first place the chemical ingredients of human calculi—uric acid and its allies—have no special affinity with the constituents which give peculiarity to drinking-water. They are animal compounds, not mineral, and it is difficult to see what hardness in a drinking-water can have to do with their excretion. In the next it is matter of notoriety that of the adults who suffer from stone a majority are precisely of the class who drink least water. Lastly and conclusively, it is impossible to assign any peculiarities to the geological formation or water-supply of the districts where stone abounds which are common to all of them, and which are not shared by others where it is rare.

Before proceeding further with speculations on this question it may be well to state the facts in somewhat more detail, and in the attempt to do this I shall avail myself freely of the excellent chapter on this subject in Hirsch's invaluable 'Handbook of Geographical Pathology.'* To take Asia first, stone is reputed to be common in Asia Minor, Syria, Mesopotamia, and Persia, but its chief seat is India.† Although met with

* See New Sydenham Society's translation, by Dr. Creighton, vol. iii., page 460.

† The facts as to calculus in India are worthy of very careful examination on the spot. It had been asserted

over the whole empire it is commonest of all in the north-western provinces. Dr. Curran, to whose zeal we owe valuable observations, holds that it is more frequent on the whole in the hill-districts than on the plains, and states that in some places it assumes the character of an endemic disease. It is extensively prevalent in Orissa, but more rare in the southern or Madras Presidency.* In Canton there is a special hospital for stone, but, excepting in this district, it is believed to be almost unknown in China. In Australia, Polynesia, and New Zealand lithiasis is almost unknown.

In Africa; Egypt, the Highlands of Abyssinia, Madagascar, Port Elizabeth, and Mauritius are places where it is believed to be fairly common; but in all other parts of this continent it is exceedingly rare.

In Europe, stone is common in France, Italy, England, Scotland, a few isolated districts in Germany, some parts of Spain, Hungary, the

that it occurred only in Mahommedans, but Dr. Curran assures us that it is equally common amongst the Hindoos. In some districts it is said that oxalate-of-lime stones are more than usually common, but, as a rule, I believe that the calculi of India are lithic acid. I have myself seen a large box-full (possibly a peck) sent home by an Indian operator, and almost all appeared to be of lithic acid. The phosphates as encrustations are certainly rare.

* As a warning against the premature acceptance of the statements of travellers, it may be mentioned that not so very long ago it was held that India enjoyed an exemption from calculus.

Moscow district, certain parts of Turkey, and a few towns and villages in Greece. It is said to be endemic in one village in Malta, whilst rare in most of the Mediterranean islands. Holland has from the earliest times had the reputation of being the special home of lithiasis, but there is reason to believe that its frequency there has recently much declined. In the following districts of Europe calculus is very rare:—Norway, Sweden, Denmark, Iceland, the whole of Russia excepting near Moscow, the greater part of Germany, all Switzerland (almost unknown), and Ireland.*

If we ask in more detail as to the prevalence of stone in England in different counties and districts, we find that the highest mortality is in the eastern counties, and the eastern and northern midlands. In Norfolk it rises to 1 in 42,744, whilst in the north-western counties it is only 1 in 209,681, or about that of Ireland generally. It must, however, always be borne in mind that mortality statistics are not a trustworthy measure of the real prevalence of lithiasis. The majority of cases of the latter attended by stone in the bladder occur in children, and are cured with but very few deaths. The mortality records concern adults chiefly. Thus it will be seen that

* Amongst the poorer classes of the Irish it may be said to be unknown. In the town of Cork there were thirteen operations in eighteen years, and in Dublin only five in each year. The mortality returns for Ireland generally show only one death to 214,740 population. The facts have been carefully examined by Dr. Yellowly.

the mortality from stone is as great in Ireland as it is in the western parts of England, whilst there is reason to believe that the number of stone cases is far less. Making, however, all allowance for this source of error, it is yet very difficult to explain the statistics which are offered us from Scotland. Here, in the birth-land of Hunter and the cradle of surgeons, we find the mortality from stone double what it is in England. It is difficult to believe that this implies a double ratio of prevalence of lithiasis, and yet it is hard to conceive why amongst the Scotch, who are good patients, and who readily resort to the excellent aid which is abundantly provided for them, the mortality should, without excessive prevalence, rise to so high a point. According to Hirsch (see page 470) the mean death-rate from stone in Scotland is 1 in 51,903, whilst in England it is only 1 in 100,331. Yet Mr. Cadge has calculated that the ratio of stone cases admitted into Scotch hospitals is only half what English hospitals receive.* As to relative local prevalence in different parts of Scotland, Dr. Hirsch could not get many facts, but there appears reason to believe that in the Aberdeenshire district stone is especially common.†

* Some further investigation of these remarkable figures is clearly needed, and an explanation of their real meaning, such as only those on the spot are competent to afford. I hope that the matter will be taken up by some of the many Scotch surgeons capable of dealing with it.

† Since this was written I have been made acquainted with some important facts collected by Mr. Plowright, which to some extent answer the questions proposed.

In the Western Hemisphere calculus is, with a few exceptions, decidedly rare. The exceptions are Canada, the city of Boston, and possibly some parts of Brazil. The negro population is not more prone to the disease in America than the same race is in Africa. Stone is said to be almost unknown in negroes. As has been already mentioned, the College Museum contains a number of mixed uric-acid and phosphatic stones removed from a negro boy in Alabama, but it is of course possible that he may have had European blood.

In the city of Boston (U.S.) there appears to be evidence of considerable decrease in the disease of late.*

One of the first questions which occurs to us in seeking to find reasons for the local prevalence or otherwise of lithiasis is whether there is a close bond of connection between it and gout. We should certainly have expected that there would be one, and the belief of English observers assuredly is that the two maladies, though not very often met with in the same person, are

* A very remarkable diminution in the number of stone cases was observed at the Bristol Infirmary in the beginning of this century. In the decennium 1750 and 1760 no fewer than 83 operations for stone were performed (eight per annum), whereas between 1810 and 1818 there were only 10 (one per annum). The diminution of stone cases in Holland has also been alluded to, and there is reason to think that similar statements may be true of many other districts, perhaps of almost all where stone has been common in former times. See Mr. Hutchison's paper, 469.

closely associated in near relatives. Thus one brother may have gout, and another, passing lithic acid in excess, may escape the results of its retention in the blood, but have a calculus form in his kidney. If, however, we regard the fact that lithic-acid stones are common in India, where gout is unknown, we are obliged to admit that there must be some law regulating its prevalence with which we are not yet acquainted. In England gout is probably far more common in London and some of the south-coast watering-places (Margate, Ramsgate, and Brighton) than it is in Norwich and amongst the agricultural classes of Norfolk; yet stone is far more common at the latter place. Gout is rare in Scotland, but I believe fairly common in Dublin; whilst the reverse holds true as to calculous disorders. The surgeons of Norfolk have exhausted their ingenuity in attempts to find an explanation for the extraordinary prevalence of stone in their county. That they have found it a difficult, or, we might almost say, an insoluble problem, is perhaps proved by the fact that one of the latest hypotheses advanced has been that it is caused by a deficient supply of milk to the children. This may be bad in Norfolk, but it is impossible to believe that in an agricultural county it is really worse than it is in many other districts, and especially in some of our large towns, which are comparatively free from stone.

If we think to find an explanation of our

difficulty by reference to peculiarity of race, and adduce the fact that negroes all over the world appear to be free from the tendency to lithiasis, we are again confronted with a host of facts on the other side. The Chinese of Canton and the Russians of Moscow suffer, whilst those of the same race in other parts of their countries escape. The populations of Norfolk and East Yorkshire, where the disease is common, are largely drawn from the Scandinavian countries, where it is almost unknown. The population of Belfast,—the city of Ireland which is most nearly free from stone,—is probably the one in which the Scotch element is largest, and in Scotland stone is abundant.

Taking a more limited view of the term race, we may admit—and it is almost the one solitary ray of light in the picture—that it seems highly probable that lithiasis is a malady which prevails from generation to generation in the same family. This fact probably to a large extent explains its prevalence in certain towns and locally isolated districts. We may perhaps even venture to use this supposition as explanatory of the facts as to the Moscow district and the several districts in Germany where the disease is, on a somewhat large scale, endemic. Some prepotent ancestor in times long past may have suffered from the tendency, and may have become the progenitor of a large section of the population. There is a feeling of improbability, however, in pushing such a supposition very

far, or in asking that it should be believed that a tendency so acquired would, in a district where the conditions were inimical, survive in intensity through many generations. It is further to be remarked that the explanation by heredity, although it may be good for the facts under present observation, does not serve us as regards the past. A man must acquire before he can transmit, and our demand is to know what are the conditions which originate the lithic acid diathesis *de novo*, or which specially favour the further development of it when acquired.

If we may argue from certain matters which are daily under our observation in English practice, we should certainly say that the free use of animal food, of beer and wine, of sugar and fruit, especially of all these together, and in conjunction with deficient exercise, favours the production of free uric acid and its allies. Many persons can, indeed, produce deposits of these salts almost at will. Especially does the addition of certain fruits favour the deposition of oxalate of lime. As regards lithiasis, English practitioners firmly believe that a diet in which animal food is restricted and green vegetables plentiful (not fruits), with abundance of exercise and water-drinking, are measures which almost certainly preclude the occurrence of the deposits in question. It is probable also that those who drink freely of diluents are far less likely to suffer from urinary depositions than those who allow the excretion to become too concentrated.

In this last remark we may perhaps have some explanation of the supposed influence of a liberal supply of good water in preventing stone. How far it may go in explaining the prevalence of stone in India, where it occurs in spite of the restricted and for the most part vegetable diet, I do not know, but it is a matter worth enquiry both there and elsewhere where lithiasis is endemic. It seems scarcely worth while to discuss the question of climate, for it is perfectly clear from the statements already made that neither cold nor heat, dampness or dryness, nor any other of the variously complicated influences which are classed together under that term, have anything important to do with the prevalence of stone.

The gradual reduction of the number of stone cases in certain places might possibly, if the facts were carefully investigated, afford valuable information. As long ago as the time of Camper it was believed that the number was diminishing in Holland, and subsequent observers* have believed that the reduction is progressive. In the city of Boston (U.S.) it may plausibly be explained by the suggestion that the conditions of life there are inimical to the diathesis, and

* In 1802 Schultens published a thesis, '*De causis imminutæ in Rep. Batava morbi calculosi frequentiæ.*' In it full statistics are given of the operations for stone in Amsterdam during the eighteenth century, showing a reduction of one-half between the beginning and the end of it. I am indebted to Professor Tilanus, of Amsterdam, for drawing my attention to this essay.

that in the first instance the unusual prevalence was due to the immigration of families in whom the malady was hereditary.

The remarkable prevalence of stone in certain parts of India is a fact of such extreme importance for elucidation that I shall deal with it in rather more detail. Many Indian surgeons have published valuable collections of facts respecting it. The following record may, I believe, be selected as giving a good illustration of the whole:—

Surgeon-Major Keelan, of Hyderabad (Sind), in a practical paper on lithotomy in the 'British Medical Journal' of October, 1887, gives us some important facts as to the prevalence of stone in that district. He operated on no fewer than 188 in one year, and has a collection of 1000 stones, which he expects, in the course of five years, to increase to 2000. All the facts as regards the ages most liable, and the relative frequency of the different constituents of the calculi, with which we are familiar in England, seem to be repeated there. 120 of the stones were uric acid, 40 the black oxalate of lime, 5 the white oxalate, and the remainder of other formations; 2 were believed to be xanthic oxide and 3 cystic oxide, but the evidence as regards these is perhaps not conclusive. 105 of the patients were under the age of twenty-five, and 163 were Mussulmans and 25 Hindoos. Out of the 188 there were only 7 females. Mr. Keelan believes that his collection shows that the

nucleus is usually of oxalate of lime. He thinks that the drinking-water may perhaps have something to do with the prevalence of stone. All his patients were, he believes, in the habit of drinking the muddy water of the Indus laden "with silt," which, as is the case with the Egyptians and the Nile water, they prefer to water made clear by filtering. It is scarcely fair to Mr. Keelan, though not closely cognate with our topic, to omit saying that of the 188 patients, of all ages, he lost but 8, and it would certainly not be fair to surgical statistics to omit qualifying this by adding that he carefully refused all feeble and very old patients.

Mr. Bransby Cooper, in a paper in the Guy's Hospital Reports, seems to have been the first to recognise the interest of the subject of calculus occurring in India, in reference to peculiarities of food and mode of life. He had thirteen calculi from natives of the Punjaub put into his hands by Mr. Coles. He found uric acid abundant and oxalate of lime rare, and was obliged, after careful investigation of the subject, to record his conviction, that "after all there are causes in operation in the production of stone altogether independent of the nature of the food." Mr. Cooper found the nucleus to consist of urates in almost all the thirteen stones. Dr. Vandyke Carter in 1873 examined the calculi in the Bombay Museum with great care. The principal result was the observation that a larger proportion in India have oxalate of

lime as the nucleus than in England, and a smaller one uric acid. Thus in an average of three English museums, uric acid or urates form the nucleus in 71·79 per cent., but in Bombay of only 56·30 per cent. On the other hand oxalate of lime is the nucleus in Bombay in 38·65 per cent., but only of 16·87 per cent. in the English museums. A very large proportion of the Bombay calculi were from natives of Bombay itself, some from the Punjaub.

Dr. Playfair, 'Edinburgh Medical Journal,' has noted the infrequency of stone in the Bengal provinces, and contrasts it with the frequency in the north-west. He attributes the difference to the use of rice in Bengal, and of a coarse wheat-flour in the north-west provinces.

Dr. V. Carter notes the comparative rarity of the mixed or fusible deposit in Bombay, a fact which my own examination of Indian calculi in our College collections fully corroborates. He thinks, however, that phosphate of lime stones (not prostatic) are more common in India than in England. "Phosphate of lime," he says, "is commonly associated with the oxalate, whilst cystitis and the mixed phosphates resulting from it are rare."

In Bombay Parsees are believed to be most liable, and Hindoos the least so. Mussulmans are more liable than Hindoos; but Dr. Carter, who struggles against the conclusion, is fain to admit with Mr. Cooper, that between the vegetable-feeding Hindoo and the meat-eating Mus-

sulman, there is not the difference as to calculous disease which might have been expected.

In the Catalogue of the Pathological Museum at Calcutta I find no mention of any collection of calculi, whilst in the museum at Hyderabad, as just mentioned, there is the imposing array of 1000. A paper on lithotomy at Calcutta, however, by Sir Joseph Fayrer, enables us to correct any impression as to extreme rarity of stone in that district. His valuable paper is characterised by the candour of statement which his writings always display. During eleven years he operated in hospital upon fifty cases, these being about half of the total during that period. Many of the patients had come to him in extreme states of illness, and the mortality was 28 per cent. All the boys recovered. The calculi were for the most part uric acid, with or without phosphatic encrustation, but there was "an occasional example of oxalate of lime." Nothing is said as to oxalate nuclei.

Amongst the possible causes of calculus in India the position which males invariably adopt for micturition has been quoted. This position is a squatting one, and it has been supposed that the additional difficulty incurred in completely emptying the bladder may conduce to the formation of stone. It is to be observed, however, that the relative frequency of stone in the two sexes appears to be the same in India as it is with us.

ON URINARY CALCULI IN THE LOWER ANIMALS.

—Urinary calculi are not of common occurrence in our domesticated animals, and are probably still less so in those living in a wild condition. The College Museum contains six from the dog, all phosphatic; seven from the hog, two phosphatic and five carbonate; seventeen from the horse, all but one carbonate; one from the ass, carbonate; twelve from the ox, all but one carbonate; whilst the sheep affords only a single specimen, and it (exceptionally) of urates. It is well known that the herbivora are liable to carbonate of lime concretions in connection with the state of alkalinity which is the normal state of the urine in them.

A specimen of a carbonate of lime urinary calculus from an ass was shown by Mr. Shattock at one of the Pathological Society's meetings, and with it one of a similar character from a dog. In connection with them Mr. Shattock remarks that carbonate of lime calculi are as frequent in the herbivora as are those of uric acid in man. It is difficult on such a point to collect statistics, but I cannot but believe that this is an exaggeration. It would be of great interest to ascertain whether calculi in the domestic animals are more common in some districts and some countries than in others.

Consistently with what we should expect from the general sameness of diet and absence of drug-treatment in animals, their calculi very rarely show any alternation of layers.

ON CALCULI OF UNUSUAL SIZE.—Amongst calculi of unusual size must be mentioned one weighing thirty-three ounces, of which a cast is in the Museum of the College of Surgeons, the original being in the library of Trinity College, Cambridge. It is of uric acid, and was removed from a woman after death. The College Museum also possesses the cast of a very large stone successfully removed by Mr. Thomas Smith by suprapubic lithotomy. It weighed twenty ounces and a half, and measured eleven inches and a half by nine and a half. A remarkable feature in this case, but at the same time one which we encounter in almost all cases of calculi of extraordinary size, whether in the kidney or bladder, is that the patient had not had much inconvenience until latterly. He was a soldier, and had been invalided only ten weeks. The stone consisted of oxalates and uric acid, with finally an encrustation of phosphates.

In 1886 Mr. Rivington operated, at the London Hospital by suprapubic lithotomy, for a stone of very large size. Its fragments weighed, after removal, twenty-three ounces and a half.

One of the largest lithic-acid stones on record is that which was removed after death, at the age of 81, from the bladder of Sir Thomas Adams. This gentleman had led an active life, and presumably had not failed in health until late, for at the age of 74 he accompanied General Monk to Breda to invite the return of

Charles II. The stone weighed 12,000 grains (25 ounces). It is now in the Museum of St. Thomas's Hospital, and by the analysis of Mr. Stewart is stated to consist of uric acid, with a small trace of urate of lime.*

In a case recorded by Mr. Southam, in which, in consequence of the known large size of the stone, he performed a recto-vesical operation, the stone (phosphatic) weighed nearly five ounces, and its diameters were two inches and a quarter by three and a third.

Weights of Renal Calculi.—Civiali states that Pohl found in the kidney of a princess a stone which weighed five pounds! Putting this case aside, Dr. Gee believed his own to be the largest on record, nine ounces being the next (see p. 80, *Medico-Chir. Trans.*, 1874).

Of calculi found in the bladder after death the following are some of the heaviest on record:—

Morand		6 pounds.
Deschamps		51 ounces.
Sir Walter Ogilvie's (phosphates)		44 „
The Trinity College specimen (uric acid)		33 „
Sir Thomas Adams' case (uric acid)		25 „
Mr. Paget's case (Leicester).		27 „

The following are some of the largest removed during life:—

Utterhævens' measured in girth	16½ in. by 12½ in.
Joseph Hunter (Madras), weighed	25 oz.
Dr. Morrison (Northumberland), weighed	1 lb. 6¾ oz.
Sir H. Thompson,	14 oz. (avoirdupois).†

* See *Pathol. Soc. Trans.*, 1870, p. 269.

† I have not attempted any exhaustive statement of facts as to calculi of unusual size, but have quoted the

ACCRETIONS ON FOREIGN BODIES.—When foreign bodies gain access to the bladder they always sooner or later become coated with deposit. The rapidity with which this is effected, and also to some extent the character of the deposit, will depend upon the state of the urine and bladder at the time. The commonest deposit is the mixed phosphates, and this occurs in connection with cystitis, the effusion from the bladder-walls of phosphate of lime, and the production of triple phosphate by decomposition of urine. If cystitis already exist, the deposit of these salts will take place immediately. A few days may, as is well known, suffice to coat thickly the end of a catheter which has been retained in the bladder. In a specimen to demonstrate this, which has been preserved in the Museum of St. George's Hospital, the catheter had been kept in only four days. If the bladder be healthy at the time a certain period must elapse, varying in different cases, to permit of cystitis being set up. Although the most frequent, mixed phosphates are not the only salts which form these concretions. In the College Catalogue a specimen is both described and figured in which a pin became the nucleus of a large uric stone, but it is especially stated that this is a most

above (chiefly from Mr. Rivington's paper) by way of examples. Some risk of error must be allowed for on account of different standards of weight.

exceptional event. Possibly, however, it is not quite so exceptional as it has been deemed, for I find that in a series of about ten specimens of foreign bodies from the bladder in the St. Thomas's Hospital Museum it is expressly stated in almost all that uric acid formed part of the concretion. In one instance the phosphates appear to have been absent, and it is stated that a needle was coated with oxalate of lime and uric acid. All depends, no doubt, upon what salts are at the time in excess in the urine. If uric acid be sufficiently plentiful to form quickly a protecting encrustation on the foreign body, the bladder may then escape the attack of cystitis, by which under other conditions the phosphates would have been produced.

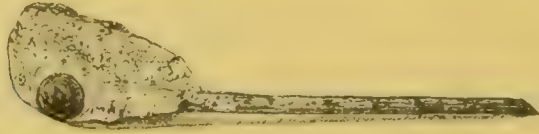
The appended woodcut shows the remarkable efficiency of the bladder contractions in doubling-



up into a sort of knot a flexible bougie which had been introduced into it. It was removed by lithotomy from the bladder of a man by Sir Everard Home, and was, when recent, encrusted by fusible phosphates.

In the next woodcut is represented a hat-pin,

which was removed from the bladder of a woman,



having a nodular mass of phosphates on one end. A much larger mass is seen in the appended figure, concreted around the larger end



of a silver bodkin. This specimen was formerly in the British Museum, and is described and

figured in Rymsdyk's 'Museum Britannicum.'
The mass consists of mixed phosphates.

The College Museum contains many other specimens of concretions around foreign bodies, but they all illustrate the remarks made above.

CONCLUDING REMARKS.

If we now, in conclusion, attempt an enumeration of the various conditions which may favour the formation of urinary stone, the following are probably the principal :—

(*a.*) First and most important of all, a tendency to the existence in excess in the urine of some constituent which is not easily soluble. This may be either uric acid, urates, oxalate of lime, xanthine, cystine, phosphate of lime, triple phosphate, or carbonate of lime. At first sight it might seem probable that the secretion in excess of one or other of these salts is almost a necessary condition to the formation of the special calculus concerned, but very little consideration is necessary to realise that it may be a question of deficiency of solvent powers in the urine, rather than of absolute excess of the salt to be dissolved. Respecting each one of the substances mentioned, there have been those who have contended that the solvent power of the fluid is by far the most important factor, if not the sole one. There can be no doubt that the truth lies midway. Cystic oxide offers us, perhaps, the most conclusive instance of the excess of a particular ingredient, leading almost of necessity to the formation of a concretion. Whilst it is quite possible that the opinion of Dr. Bird, partially sustained by Dr. Prout, may

be correct, that many persons have a small quantity of cystine in the urine, it yet appears to be highly probable that whenever it is present in any degree of abundance, the formation of a concretion is an almost necessary result. What is true of cystine is probably true in less degree of uric acid, oxalate of lime, and the rest.

(b.) Next in importance to excess of some calculus-forming ingredient, must be mentioned deficiency of the solvent. Although some of the substances which form stones are when pure but little soluble in water, there is no doubt that they all are more or less so when developed in a nascent condition, and in association with other urinary ingredients. It therefore becomes of the utmost importance that the total quantity of the urine should be adequate. Thus a supply of pure water and a systematic cultivation of the habit of drinking it in liberal quantities, is probably one of the best means of diminishing the frequency of stone in any given community, and in seeking to explain its prevalence or infrequency in any stated locality, primary attention should be paid to this point. The solvent properties of the urine may, however, be increased irrespective of its increase in quantity. Thus a sufficient supply of alkalies, whether taken as such or in the form of green vegetables, is a very important means of promoting the solution of some of the commonest ingredients of calculi. There is also good reason to believe that common table-salt possesses great efficiency in this direction.

Its consumption diminishes indirectly the deficiency of urine, prevents the formation of colloids, and very much helps the solution of the compounds of uric acid. This is a point which in India, Moscow, Norfolk, and other places is worthy of the most serious attention.

(c.) In the third place must be mentioned conditions which favour the formation of nuclei. It is well known that patients may pass uric acid, oxalate of lime, or phosphates in great excess for long periods, yet never get stone, on account of the media not being favourable to cohesion of the particles. Any inflammatory attack may, by supplying mucus, become the determining cause of a concretion in any part of the urinary apparatus. So probably the development of oxalate of lime in the urinary tubules in a patient who is habitually passing uric acid, may serve as a centre of concretion. Certain colouring matters may do the same. Foremost, however, among the causes of the formation of nuclei, must be placed all influences which tend to prevent complete emptying of the bladder. If the urine is left stagnant, aggregation of its salts in concrete masses is greatly favoured; and it can be easily understood that this is especially the case, when the last portion of the urine is habitually retained. The difficulty of emptying the bladder completely in enlarged prostate is probably one of the chief causes of the frequency of stone in men past middle age; whilst the ease and certainty with

which the bladder is completely emptied in women is probably one of the chief causes of their comparative exemption.

In mentioning an excess of certain insoluble substances in the urine as being obviously one of the chief causes of concretions in the urine, it is clear that we take but one little step. The important question remains, how do those substances come to be present in excess? The answer to this question is extremely complex, and branches out in various directions. It would not be profitable to attempt to enumerate more than a few of the chief.

Excesses of certain elements of food or defects in the quality of the food, or defects of the assimilative power of the individual are no doubt of great importance, and they will be found treated of in ample detail in many of the excellent works which deal with this topic. It is, however, far more difficult than some of the authors of these works would appear to have realised, to establish satisfactory laws in respect to them. We may say at once, that there can be no doubt that hereditarily transmitted peculiarity of organisation is very efficient in giving tendency to stone. This peculiarity may be quite independent of everything that can be designated as ill-health. Many patients who pass uric acid or oxalate of lime, or who become the subjects of calculi consisting of these substances, appear to be leading very judicious lives, and to be in the enjoyment of

excellent health. This fact is not unfrequently emphasised in cases in which a tendency to calculus prevails in families, and it is especially well shown in the case of the cystine calculus. It may be that the transmission of hereditary peculiarities as regards assimilation will explain a good deal as to the local prevalence of stone, and that it should be considered rather a family prevalence than an endemic one. It has been suggested that in those localities in Germany where stone appears to be restricted within very narrow areas, it is hereditary descent rather than dietetic or telluric conditions which determines its prevalence. We must not, however, push this doctrine too far; it is obvious that all hereditary diseases have been acquired in the first instance, and in a general way the tendency to them dies out unless the influences under which they originated are continually renewed. We must remember also, that in certain districts, inhabited by different races, there appears to be but little difference in the members of each as regards proclivity. Thus in India, Parsees, Mussulmans, and Hindoos, all suffer. Those who have investigated the facts as to the predisposition to calculus in England have almost unanimously believed that inherited tendency to gout is an important factor. Probably they are right to some extent, but how explain in this hypothesis the remarkable prevalence of stone in Norfolk, where gout is not more common than in other parts of England? Or, above all,

how explain its frequency in parts of India where gout is unknown? The difficulty of explaining the prevalence of stone in India by reference to any peculiarity in diet has already been adverted to in some detail. And when we remember that it is almost unknown in negroes, the problem becomes almost insoluble. Had it been prevalent throughout the tropics in equal frequency, we might have believed that the too profuse action of the skin, by favouring concentration of the urine, was an important factor. But the exemption of many large tracts and some entire races discredits this suggestion.

The possible influence of the skin on the urine, by producing concentration, has been referred to in the preceding paragraph. Although it is scarcely consistent with facts, to suggest that high temperatures and the profuse sweating induced, are causes of the endemic prevalence of stone; yet it may be reasonably suspected, that when a definite proclivity exists, this influence may be importantly influential. Thus it is very possible in the inhabitant of a temperate climate, with an habitual tendency to the formation of lithic acid in excess, that the occurrence of dry hot weather, and its well-known attendant, concentrated urine, may be the turning-point as regards the formation of a concretion. It has been believed that certain skin-diseases, Eczema and Psoriasis for instance, are not infrequently the accompaniments of a tendency to stone. But it is very possible that the statements

on this matter have been made on insufficient data.

In conclusion, we must not allow the consideration of details, such as those which have engaged our attention in the preceding pages, to conceal from our view the main facts. Whatever may be the importance of the precise mode of the formation of nuclei; however true it may be that local conditions of the urinary organs may favour the formation of stone, the fact remains, that a large majority of calculi must be regarded as primary, and as evidence of the existence of diathetic tendency. These tendencies, originating it may be in peculiarities of diet, climate, mode of life, &c., have become in the course of time hereditary, and are now the appanage of certain families and races. They are susceptible of modifications by the life-habits of the individual, but they are, when strongly marked, to a large extent independent of them. The urinary disorders which result in calculus are, for the most part, simply expressions of peculiarities of assimilation. In diabetes* such a peculiarity

* It is well known that the diabetes of aged persons is often coincident with lithiasis, and were it not that the abundance of the solvent prevents the salts from showing themselves, concretions would probably be oftener noticed. Diabetes is a malady the frequency of which varies much in different countries, and some of the very imperfect facts which we possess, would appear to indicate that it prevails in the same communities as those liable to stone. Thus our earliest descriptions of it occur in Indian literature; it is said to be unknown amongst the

finds its conspicuous expression in the presence of sugar with great excess of water; in lithiasis, in the presence of lithic acid, lithates, lithic oxide, oxalate of lime, with no excess of water; in the cystine diathesis, in the presence of cystine. No doubt a number of other diathetic states exist, modifications of some extent of the above, and only less common and less conspicuous than them. That a phosphatic diathesis in this sense of the words must still hold its ground seems highly probable; and since in the herbivora one such is found which expresses itself by the excretion in excess of carbonate of lime, it is highly probable that in rare instances we may recognise the same in man.

It is quite impossible to associate immediately, that is in the individual, these diathetic states with peculiarities in diet, climate, or habits of life, though all these may have remotely their share of influence in predisposing to or aggravating the condition when once formed. Nor, although some of them are closely allied, is it true that any one is identical with the constitutional condition known as gout. We must regard both gout and the various urinary diatheses as resulting from the hereditary trans-

negroes, and it is nearly twice as frequent in England as in Ireland. It is common in Norfolk, where stone is common; it is unknown at St. Petersburg and Copenhagen, and at both these places lithiasis is very rare also. (See Hirsch, vol. ii. p. 646).

mission through many generations of peculiarities which have gradually assumed more or less of individual type. Whilst clearly admitting these facts as constituting the general basis of our creed, we must, on no account, lose sight of the other collateral ones which concern the modifications of the inherited diathesis in the individual who is its subject.

As regards the prevention of stone in districts where it is prevalent, we may, perhaps, at the risk of repetition, be permitted to insist on two paramount rules of diet: the free use of drinking-water and the free use of table-salt.

POSTSCRIPT.—Those who desire fuller information on the subjects briefly adverted to in the above Essay, should refer, amongst others, to the following works:—

Marcet on Calculous Disorders. 1819.

Prout on the Nature and Treatment of Diabetes, Calculus, &c. 1825.

Prout on Stomach and Renal Diseases. 1848. Containing an admirable summary of statistical information as to calculus.

Owen Rees on the Blood and Urine in Health and Disease. 1845.

Lionel Beale on the Urine. [Dr. Beale's very able work is, I think, the only one which recognises the possible importance of the salt question. He denounces the cruelty of taxes on salt.]

Ralfe on Morbid Urine. 1882.

Carter (Dr. William) on Renal and Urinary Diseases.

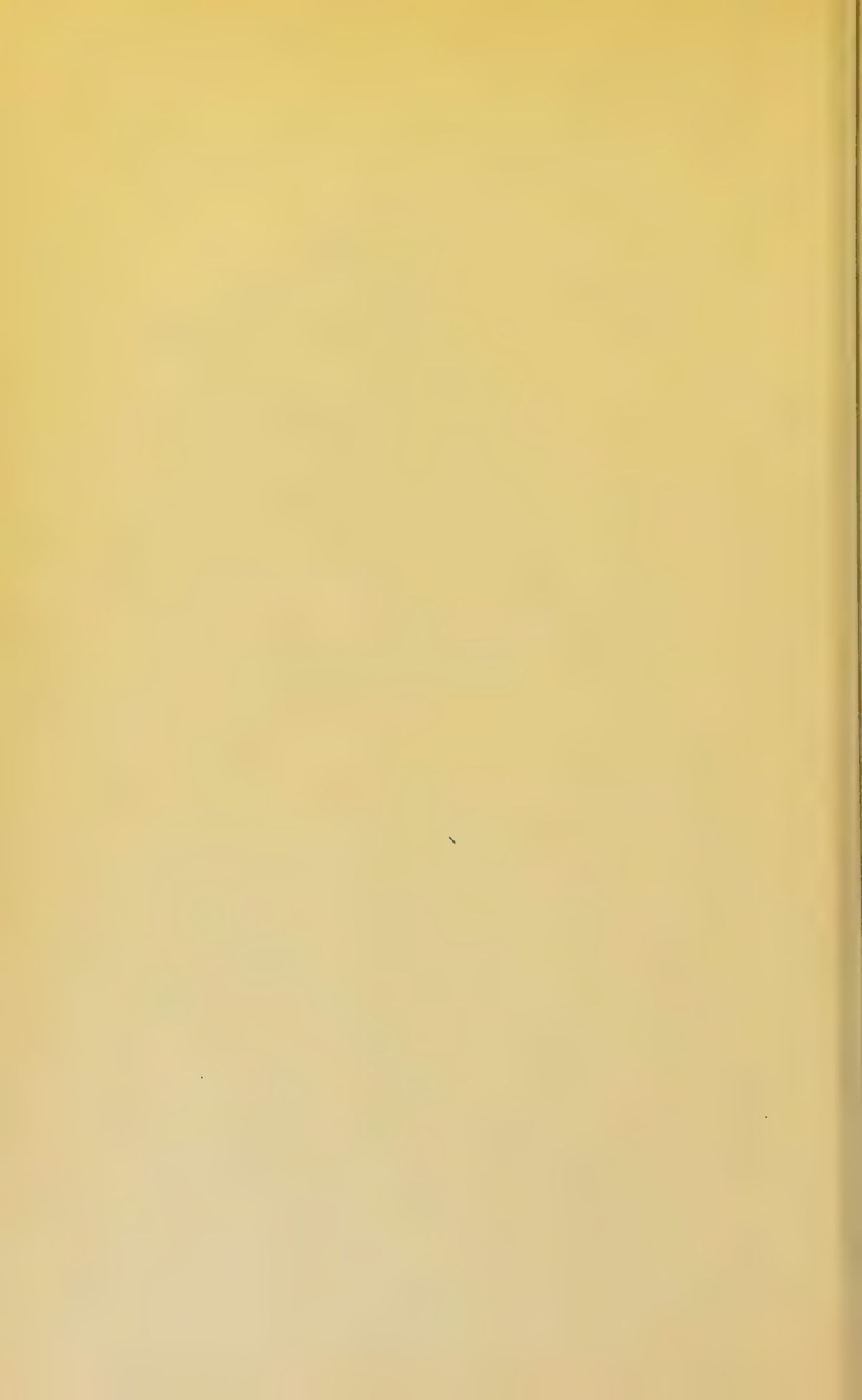
Carter (Dr. Vandyke) on the Microscopic Structure, &c., of Calculi. 1873.

Sir William Roberts on Urinary Diseases.

An excellent summary of most of the above will be found in a recent (1884) work, as yet not translated into English, by Dr. Wilhelm Ebstein, of Gottingen, under the title of 'Die Natur und Behandlung der Harnsteine.'

The Catalogues of the Royal College of Surgeons, from which I have quoted so freely, are full of valuable information, and are liberally illustrated. The reader must be careful not to overlook the fact that in addition to Parts I. and II. there is a supplementary volume.





Some Particulars of a Case of marked Bronzing of the Skin. By JONATHAN HUTCHINSON, Surgeon to the Metropolitan Free Hospital.

The following are the brief notes of the illness of the patient whose condition the Fellows will presently have an opportunity of observing:

Emma B—, æt. 34, is a married woman, but childless. She states that on her mother's side there is an hereditary tendency to consumption, and that she has herself always been considered delicate. Fifteen years ago she was for some time an in-patient at Guy's Hospital, under Dr. Bright's care, for what she was told was "disease of the kidneys." From this she recovered, but continued weakly and dyspeptic, until about four years ago, when her health began more decidedly to fall off. One of the earliest symptoms was morning headache; she also got thinner and weaker, and suffered from pain across the lower part of the back. Her friends told her she looked ill, and said she had the jaundice. She has now been for three years continuously under medical care; but states that she does not recollect to have noticed any decided change in colour until about two years ago. That it has been of longer duration is, however, rendered probable by the fact that my friend, Mr. Bletchley, under whose care she was admitted at the Finsbury Dispensary in April, 1856, at once noticed a characteristic degree of bronzing. The change in hue commenced, as far as she is aware, on her hands and face. Coincidentally with its development she has slowly lost flesh and strength. The appetite has usually been very bad, and the bowels inclined to be costive. The catamenia have been regular until quite latterly. The urine has sometimes deposited a lateritious sediment on standing, but more usually has been quite clear. She has suffered greatly from a feeling of general malaise and

as deep as that of a very dark mulatto. "Her muscular strength," adds the writer, "was exceedingly impaired, and she felt an indescribable malaise and restlessness, for which she could assign no reason. She was easily put out of breath by any corporeal exertion, though she could expand her chest and take in a very large volume of air without the least difficulty." We have here a good description of the prominent features in the cachexia of diseased capsules. At the autopsy every organ in the body was found "in a state of the most perfect integrity, except the heart." As to the heart, however, all that is described is, that its right ventricle was thin and distended, its left closely contracted, and having very thick parietes. Now as there was no disease whatever of the valves, and as the heart does not appear to have been enlarged, we may, I think, assume that the condition described was nothing more than that state of death-contraction in former years so often mistaken for "concentric hypertrophy." As there is further no note of the supra-renal capsules having been inspected, the Society will, perhaps, not accuse me of discourtesy if I venture to hint that they were probably not looked at, and express a suspicion that they were the organs really at fault.*

* I am indebted to my friend Dr. Wilks for drawing my attention to this interesting case.



NOTES ON A FORM OF ACNE LUPUS

WHICH OCCURS ON THE SHOULDERS IN CONNECTION WITH LUPUS
ERYTHEMATOSUS OF THE SCALP.

By JONATHAN HUTCHINSON, F.R.S.

I HAVE now seen three cases in which patients, who had long been the subjects of lupus erythematosus, finally had their shoulders spotted with little acne-like pustules, some of which assumed the conditions of lupus. In all three cases the scalp had been affected. I do not think that this condition on the shoulders and back has ever been described, and it is one of great pathological interest and importance. When lupus spreads on the hairy scalp, it is probably an affection chiefly of the adjuncts of the hair-follicles; in other words, it is an ally of sycosis. Now, when these spots to which I refer appear on the shoulders, they are obviously of the acne kind and are developed from sebaceous glands. They affect precisely the localities in which acne occurs. It is, therefore, not at all improbable that they result from infective elements derived from the sebaceous glands on the scalp. The first of the three cases to which I allude was that of a young woman who had been under Dr. Colcott Fox's care, and who was brought to me by an artist who was anxious to make a sketch. With Dr. Fox's permission a drawing was made, which I now possess. It shows the back of the neck and upper part of the shoulders covered with little papules. In this case none of the spots were pustular, and the patient had no special tendency to

common acne. Dr. Fox was much interested in the case, and took the patient to the Dermatological Society.

My next case was that of a gentleman named G——, who had suffered from boyhood from a very severe form of lupus erythematosus of the scalp. He was about 25 years of age when he came under my care. I had had him under observation for more than a year, when he one day told me that he had some spots on his back. He had been liable to acne more or less for years, and was accustomed to take but little notice of the spots on his back, which had only lately given him trouble. On making him strip I found the conditions, which are well shown in a portrait which I possess, and of which there is a duplicate in the College of Surgeons' Museum. Scattered amongst many little pustules, which could not be distinguished from common acne, there were a few patches, the size of a sixpence, made up of groups of little red lichen papules, which were clearly infective. Some of these showed evidences of scar formation, and others little tufts of dilated capillaries, exactly resembling those on his scalp. The patches, which I have described, are seen in the portrait, one on the back of the neck, one just behind and below the left acromion, another on the same side near the posterior border of the scapula, and three or four on the right side of the back and on the back of the right upper arm.

My third case is that of a lady named T——, whom I have had under care for lupus erythematosus, affecting both the face and scalp, for the last six or seven years. All her patches of lupus are now in a condition of healthy scar, with the exception of one very little one on her cheek. Cure has been effected by repeated cauterizations. She has lately been very liable to flush in the face, presenting conditions not to be distinguished from acne rosacea. Upon her shoulders, however, there is a number of little acne spots, which, spreading at their edges, have taken on the conditions of lupus. They are exactly like those delineated in the portrait which I have described.

These cases appear to me to have especial interest as illustrating the general law that infective elements, set free by disease in any one tissue or viscus, tend to find their home and to develop in some analogous structure. On this theory I think we may plausibly

explain how it is that the sycosis-lupus of the scalp is attended by acne-lupus of the shoulders. As yet I have only seen the shoulders affected when the scalp suffered, but it is very possible that if we were to examine more frequently the shoulders of other patients suffering from lupus erythematosus, we should find that the conditions I have described are not very uncommon. In many forms of lupus erythematosus the sebaceous glands are very definitely affected.



Wanted a test of the paper
a paper on Stomatitis in
Med: Clin: Cases

